DOCUMENT RESUME

ED 056 062 TM 000 852

AUTHOR	Beall, Geoffrey
TITLE	Change-Over Experiments in Practice.
INSTITUTION	Educational Testing Service, Princeton, N.J.
REPORT NO	RB-71-38
PUB DATE	Jun 71
NOTE	386p.
EDRS PRICE	MF-$0.65 HC-$13.16
DESCRIPTORS	Analysis of Variance; Computer Programs; *Data Analysis; Experiments; *Guides; Mathematical Applications; Mathematical Models; Mathematics; *Research Design; Research Methodology; *Research Problems; Research Tools; *Statistical Analysis

ABSTRACT

This guide presents many new experimental designs for data analysis and reviews certain standard procedures. It is hoped that the treatment of the material will stimulate new applications since the emphasis of the guide is on the problems encountered in running experiments rather than on an elaboration of mathematical models. Discussed are: change-over experiments; Youden fields; carry-over of treatment effects; writing Youden designs; analysis of data from latin squares, from a single Youden (txcxt) and from various special kinds of Youden; designs involving pairs or a single change-over; and the general problem of misfill. (CK)

RESEARCH BULLETIN

ED056062

TM 000 852

RB-71-38

CHANGE-OVER EXPERIMENTS IN PRACTICE

Geoffrey Beall

U.S. DEPARTMENT OF HEALTH, EDUCATION & WELFARE
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Educational Testing Service
Princeton, New Jersey
June 1971

Slight, in every sense, assuredly it will be. Probably in the same way it will be superficial; but when I used the word I was thinking chiefly of its most modern connotation. I meant that I was going to try to be intelligible. I sympathize with those writers who have been obliged by poverty or the exigences of military service to dispense with education, and I quite understand why they discountenance those whose object it has been to express ideas as simply, clearly, and briefly as possible. Such desperate methods would reduce the longest books of many of our best prophets to a very few pages; for when there is no butter to spread you cannot even spread it thin. In such dearth the only thing to do is to dig mysteriously into the loaf, which in literature is called being profound. And though there are readers who, having gone down to the bottom of the pit and there failed to discover the smallest speck of margarine, will venture to call such profundities empty, in the brisker parts of Europe and America the profound style is generally held in honour. In me, however, the airs of a mole or a miner would be mere affectation. Besides, unlike modern poetry and philosophy and philosophic fiction, an essay of this sort cannot hope to appeal to that great public which, in quest of life, brushes aside all hair-splitting distinctions between sense and nonsense. I dare not be profound. And frankly this essay would have been written with all the shallow lucidity of Montesquieu, Hume or Voltaire had the essayest known the secret of their superficiality.

Clive Bell in "Civilization." New York: Harcourt, Brace, 1928. Pp. 20-21.

Foreword

A Change-over experiment is one where several Treatments are tried in succession on a single unit; there are, of course, a number of such units. The unit may vary from being an animal under several diets in succession, a patient under several sedatives successively, to a machine under successive methods of handling. It is, indeed, astonishing in what varied fields it is convenient, profitable, or even inevitable that one employ Change-over. Applications arise in Medicine, product testing, Animal Husbandry, Industry, etc. Where may one not find questions best studied by submitting a given unit to 2 or more successive treatments?

The present book embodies many new experimental Designs and looks at certain questions in a fresh way. Mainly, however, it is written to set forth the good things that are available but not being used. It is hoped this endeavor may lead to application and even to progress. The emphasis is on the problems encountered in running experiments, rather than on the elaboration of mathematical models.

The discussion can be followed by anyone with the elements of algebra. It will seem the more pointful if the reader has already experienced the difficulties of making experiments work. It is, indeed, to people who do carry out experiments, or seriously wish to do so, that this work is addressed. When the writer was doing so, the present book was what he wanted on his shelf.

Change-over Experiments in Practice

Page

I. The Change-over experiment 3

II. Exploration of Youden field 31

III. The Carry-over of Treatment effects 38

IV. Writing Youden designs 57

V. Analysis of data from latin squares 99

VI. Analysis of data from a single Youden, txcxt 180

VII. Analysis of data from various special kinds of Youden 235

VIII. Designs involving pairs or a single Change-over 283

IX. Epilogue--Misfill 334

I. The Change-over experiment

Page

Change-over comparisons in practice 4

Changes-over involving 2 Treatments per unit 8

Changes-over involving rather more than 2 Treatments per unit, i.e., Youden rectangles 13

Double Youdens 19

Yates rectangles 23

Latin squares 24

Metalatin Designs 27

The historic limitations of experimental design 29

Change-over comparisons in practice. Very briefly, before the question proper of Change-over is considered let it be understood that we are concerned here with experimental Design, which involves the arrangement of tests or questions, in space, in time, or on entities, in order to discover the effect of experimental Treatments. To most people--to most people even in the sciences--the problem seems a trivial one. If one wants to know the effect of some Treatment, one simply applies the Treatment and observes the effect. In fact, it is not so easy. Inconsistencies in response whether by men or machines become confused with true complexities in the response. That is why there are statisticians or perhaps with less logic, but clearer connotation, biometricians. There are problems other than the aforesaid confusion, but let us not bother with them; rather, let us hurry to relief from them. The problems will become apparent in their relief.

A Change-over experiment is a restricted class of experimental design when, for example, each of 2 kinds of sedative are tried in successive periods on each of a number of patients. The length of time each patient sleeps under each kind of sedative is contrasted. This procedure differs from an experiment where kind (1) of sedative is given to some patients and kind (2) is given to other patients. Then the time patients sleep under (1) is contrasted with the time the other patients sleep under (2). This is not a Change-over. The distinction made may be familiar to many readers in connection with discussion on paired and unpaired t-tests. In general, when possible, the change-over experiment is recommended as giving the sharper contrast between 2 kinds of sedative, because then only the inconsistencies, from time to time, in a man's response embarrass the effect of

Treatment. Otherwise, it suffers the greater embarrassment of the inconsistency from man to man.

It is obvious, of course, that in Medicine it has long been the practice to try alternative Treatments, such as sedatives, on a given patient. And this is a legitimate and valuable experimental technique. The matter under discussion in the present book is the trying of several Treatments, successively, on a patient and the policy to be pursued under such conditions as perhaps there being fewer alternatives possible than there are Treatments. Such questions have not, however, to the writer's knowledge been pursued so far in Medicine as in Animal Husbandry. There change-over experiments are much used. Thus one may use several diets or medications on a given cow because she is more like herself from time to time than she is like other cows. The comparisons of the Treatments are subject only to the inconsistency or variability within a cow rather than from cow to cow. The situation is, of course, complicated by the possibility that there may be lingering effects, and it is with this possible complication that the present book is largely concerned.

The considerable experimental advantages of Change-over experiments have been sought after with the most effort in Animal Husbandry, but apparently are as real in other fields. Statistics is a curious thing--the same problems arise in such various fields. To meet a considerable variety of problems both in Agriculture and in Industry the writer has taken advantage of Change-over experiments. In industrial experimentation, the experimental units, such as a machine to make something, resembles itself from time to time much more than it resembles alternate units--as in distant Animal Husbandry. In such cases, one is inclined to comparisons of Treatments (or methods or materials) within units. One is inclined to try 2 or more Treatments (or methods or materials) on a given Machine in order to compare them. Such change-over design is perhaps particularly suitable to machines which continue much the same for fairly long periods, in contrast to, say, cows, which are subject to lactation cycles and other major trends. If it is a matter of a subject to try various Treatments, the argument is the same. In this book the author sets forth the more simple and productive aspects of such a procedure; much chaff has been avoided. The exhaustive exploration of an idea, as is proper in a theoretical study, has not been attempted. On the other hand, it has been attempted to cover and illustrate such ideas as seem profitable very fully. It is hoped that the author's experience may be useful to workers in the fields mentioned and very probably in the Social Sciences.

The discussion, so far, while true enough, must be tending to create the misconception common from the literature, i.e., that change-over experiments are to be justified by their efficiency. And a shrewd man may say he prefers simplicity to efficiency. In fact, one may have change-over experiments forced upon one, as it has been said "some men have greatness forced upon them." Thus Industry has often had change-over experiments forced upon it. By way of a simple example, suppose one had 4 different Treatments; suppose further that one wanted to try each of them about three times. One might not be able, like the classical agronomist, to assign 12 Machines to the problem, giving each Machine some one of the Treatments--this because one has, in fact, only 4 Machines. Perforce, one must do something like assigning 3 Treatments to each Machine, one is in a change-over experiment. Such a problem in the extreme forced itself upon the author in his work on paper making. There, due to practical considerations, he was restricted to a single great paper-making machine. He was forced accordingly to arrange a change-over experiment where a succession of bactericides was tried on the recirculated water of one great machine in an experiment spread over weeks. This was change-over operation in the extreme.

The idea, as initially proposed, of comparing the Treatments (1) and (2) on each patient can be extended to a whole sequence of designs. First there may be more than 2 Treatments, say (1), (2) and (3), of which one patient tries (1) and (2), another (2) and (3) and a third patient (1) and (3). Such may be termed paired comparisons. In various forms they are in wide use. They are discussed in the next section. One may go on to cases termed Change-over in Youden rectangles, where several, but not all, Treatments are tried by each subject, patient, or machine. As discussed in the section, after next, one may, given 7 Treatments, try 3 on each of 7 subjects, patients or machines. Alternatively one might try 4 Treatments on each experimental unit. From these one may go on to change-over latin squares, where all Treatments are tried on all experimental units. These are discussed below. All these Designs arise very naturally as extensions of the fundamental idea of paired comparisons. The most attractive and easily handled of the designs is the Latin square--all Treatments by each experimental unit--but it is unfortunately often not practical. If one wanted to try 7 kinds of sedatives each for 2 weeks it would be inadvisable to use a latin square because before 14 weeks were out many of the participants would have been lost by death or recovery. Accordingly, one would try a Youden rectangle which would require only 6 weeks. Again a subject, in order to try a Treatment fairly, may have to use it 3 weeks. Then it is practical to try only a few Treatments on a given subject on account of this consideration of time. Since, however, there are many people, a great deal of information can be gathered fairly shortly. Accordingly, one uses a Youden rectangle. In industry, not only are experimental subjects liable to death or discharge, but experimental results may be required very shortly--"We must have the results within 5 weeks." Such press may make it difficult to try many Changes-over on a machine.

Changes-over involving 2 Treatments per unit - In the realm indicated a very simple and oft-used Design is the paired comparison of Treatments (1)...(t) , 2 at a time. To be at all satisfactory the Design should compare each Treatment with every other Treatment, at least once. It should be at once indicated that, to the contrary, there is extensive discussion, in the literature, on paired comparisons where a judicious subset of comparisons is made. An example is the writings of Cox (1958), which is discussed under the heading of missing data in Chap. VIII. The concept that it is best to make all comparisons an equal number of times is based on two notions. First, such comparison will be of comfort to the "practical" man. Secondly, any other arrangements will give rise to elaborate allowances for the various accuracy of the various comparisons. For the situation of 2 Treatments per unit, accordingly, the Design requires at least $t(t - 1)/2$ pairs, since there are that many comparisons. Thus for $t = 7$ Treatments tried on 21 Men, each Treatment to be tried for one Day, we may write the Design as shown in Table Ia. It is, of course, because $t(t - 1)/2$ may become inconveniently large that experimentalists take refuge in the judicious subsets indicated above. Since the two Treatments are to be tried successively the order of trying may be important. Then all Treatments were best tried equally on the first and second Days, or in the first and second Columns.

It would not do to assign some particular Treatment largely or wholly to a given Day which might be the favorable or unfavorable day. The treatment and day effects would be confounded. Thus, if in Table Ia the Design had been such that Treatment (2) fell always in Day 2 and, if that Day the response had been particularly favorable, (2) would appear to considerable but false advantage. As matters stand Treatment (2) occurs equally in Days 1 and 2 and so any favorability of Day 2 must be balanced by the unfavorability of Day 1. For 21 Treatments such balance can be managed in the 21 pairs. In general for t, odd, it can be managed in $t(t - 1)/2$ pairs. The situation for t, even, is discussed below.

The fundamental issue of the Design of Table Ia can be seen most simply in the form of what may be termed a comparison table as shown in Ib. It is something like the schedule of games of an athletic league. In this comparison table, x indicates a comparison and o indicates its repetition in the opposite order. The form of expression depends on whether one says (3) vs. (4) or (4) vs. (3). Thus the x in the second Row and first column arises from Man I, who tried both Treatment (1) and Treatment (2). At the same time the o in the first Row and second column, also arises from Man I. Since all cells, off the principal diagonal, are filled once we may say the comparison table has fill of 1. One might, of course, employ some multiple of 21 pairs, such as 63 pairs, to get the comparison table filled thrice. In the business of designing the experiment, one would simply write out the arrangement of Table Ia three times over. It may be added that since Treatment (1) occurs thrice on the first Day and equally on the second Day the totality of comparisons

Table I - Comparisons of 7 Treatments in 21 pairs

a. The Design by Men, Days and Treatments

Man	Day 1	Day 2	Man	Day 1	Day 2	Man	Day 1	Day 2
I	(1)	(2)	VIII	(1)	(4)	XV	(1)	(6)
II	(2)	(3)	IX	(2)	(5)	XVI	(2)	(7)
III	(3)	(4)	X	(3)	(6)	XVII	(3)	(1)
IV	(4)	(5)	XI	(4)	(7)	XVIII	(4)	(2)
V	(5)	(6)	XII	(5)	(1)	XIX	(5)	(3)
VI	(6)	(7)	XIII	(6)	(2)	XX	(6)	(4)
VII	(7)	(1)	XIV	(7)	(3)	XXI	(7)	(5)

b. The comparison table

Treatment

vs.	(1)	(2)	(3)	(4)	(5)	(6)	(7)
(1)		o	o	o	o	o	o
(2)	x		o	o	o	o	o
(3)	x	x		o	o	o	o
(4)	x	x	x		o	o	o
(5)	x	x	x	x		o	o
(6)	x	x	x	x	x		o
(7)	x	x	x	x	x	x	

c. Differences in percentage Satisfaction achieved by 7 types of Treatment given to 21 Men

Treatment

vs.	(1)	(2)	(3)	(4)	(5)	(6)	(7)
(1)		-3	-3	-2	+4	-9	-9
(2)	+3		+11	+4	+5	+2	-2
(3)	+3	-11		-10	+1	-15	-2
(4)	+2	-4	+10		+5	-3	+8
(5)	-4	-5	-1	-5		-12	-4
(6)	+9	-2	+15	+3	+12		0
(7)	+9	+2	+2	-8	+4	0	
Sum	+22	-23	+34	-18	+31	-37	-9

of Treatment (1) with others can be nowise influenced by the one Day being better than the other.

Table Ib is the situation as it might be conceived at the time of laying out the experiment; Table Ic shows the results after completing the experiment. Each Man stated his percentage satisfaction with each Treatment given him. The material question is, of course, the difference in that satisfaction between the two Treatments. Thus Man I gave Treatment (1) a percentage satisfaction 3% greater than he gave Treatment (2). Accordingly, there is entered in the second Row and first Column of Table Ic the value +3. By the same token, there is entered in the first Row, second Column, the value -3. Treatment (2) lost to (1) by 3%. It is noteworthy that Treatment (5) was given a higher rating than any of the 6 competitive Treatments, and this by 6 men. On the other hand, Treatment (2) was only once rated higher than any other Treatment, and the other 5 times it competed was rated lower. To return to the example of the athletic league, we should say that a team that always, or almost always, got more goals than the others was probably a good team. Certainly we should prefer to put our money on team (5) rather than on team (1). For Table Ic the situation is, obviously, fairly well summarized by the sum beneath each Treatment.

The design of paired comparisons for t, even, is not quite as simple as writing down all comparisons of t things 2 at a time, i.e., $C_2^t = t(t-1)/2$. The problem is that there will occur $t - 1$ pairs involving any given Treatment like (1), and it is plainly impossible to dispose an odd number of things equally between 2 Columns. In some measure Column effects would be confounded with the effect of Treatment (1). In order to arrange the Treatments so that each occurs the same number of times in each Column--the minimum number of pairs or Rows is $t(t - 1)$, i.e., twice as many as for the case of t, odd. The matter may be illustrated for a test on $t = 4$ Treatments as follows:

	Period			Period	
Row	1	2	Row	1	2
I	(1)	(2)	VII	(3)	(1)
II	(2)	(3)	VIII	(4)	(2)
III	(3)	(4)	IX	(1)	(4)
IV	(4)	(1)	X	(2)	(1)
V	(1)	(3)	XI	(3)	(2)
VI	(2)	(4)	XII	(4)	(3)

This Design double-fills the comparison table. Thus any 2 Treatments such as (1) and (2), are compared twice. One might, of course, employ some multiple of 12 pairs, such as 36 pairs, to get the comparison table filled 6 times. The results would obviously be more reliable with so much more data.

It seems undesirable to go further here into the question of paired Designs. It seems better to look at the whole field of change-over experiment and then return in a very thorough way to paired Designs in Chapter VIII.

There is associated with paired Designs, as just discussed, and for that matter with all the Designs that will be discussed, what is, for present purposes, a degenerate class called balanced incomplete blocks. These are situations where only the comparisons are of importance, and the order within the pairs is unimportant. Such may well be the case in the spacial Designs of Agronomy where the two members of a pair are simply two little plots of ground. Such cannot generally be the case for change-over experiments. Accordingly, the following discussion will be concerned only with Designs in the class where the Columns are important. In such a case, the little distinction between n even and alternatively odd disappears, since there is no problem of balancing the Treatments between the 2 Periods, or Columns.

Changes-over involving rather more than 2 Treatments per unit, i.e., Youden rectangles - Most people must be aware of the possibilities, just briefly discussed, of writing paired comparisons but certainly many may not at all have considered the possibilities and profit of trying more than 2 Changes-over on a given unit. Such Designs are called Youden rectangles. For the moment consider them as cases where the number of items per Row, i.e., the number of Columns is $2 < c < t$, the number of Treatments. For instance, suppose one wanted to test 7 Treatments on 7 Machines, each of which was to be subject to 4 of the Treatments. Then, one may write some Design such as that shown in Table IIa. Since this has $t = 7$ Treatments in $c = 4$ Columns for $r = 7$ Rows, it may be called a $t \times c \times r$ or 7x4x7 Youden. In any Row there are generated, implicitly, by a given Machine, 6 comparisons. Thus Machine I yields the comparisons (1) vs. (2), (1) vs. (4), (1) vs. (7), (2) vs. (4), (2) vs. (7) and (4) vs. (7). All such comparisons double-fill the comparison Table IIb. Thus the present is balanced Design, in contrast to the class of partially balanced. In a Youden rectangle, at least for the type used in the present book, the number of Rows is always the same as the number of Treatments although the number of Columns may vary. The Rows in this Design might, in the widely used terminology that originated in Agronomic experimentation, be called blocks. The term has, however, little

Table II - Comparison of 7 Treatments in sets of 4

The 7x4x7 Youden

a. The Design by Machines and by Periods

Machine	Period 1	2	3	4
I	(1)	(2)	(4)	(7)
II	(2)	(3)	(5)	(1)
III	(3)	(4)	(6)	(2)
IV	(4)	(5)	(7)	(3)
V	(5)	(6)	(1)	(4)
VI	(6)	(7)	(2)	(5)
VII	(7)	(1)	(3)	(6)

b. The comparison table

vs.	Treatment (1)	(2)	(3)	(4)	(5)	(6)	(7)
(1)		oo	oo	oo	oo	oo	oo
(2)	xx		oo	oo	oo	oo	oo
(3)	xx	xx		oo	oo	oo	oo
(4)	xx	xx	xx		oo	oo	oo
(5)	xx	xx	xx	xx		oo	oo
(6)	xx	xx	xx	xx	xx		oo
(7)	xx	xx	xx	xx	xx	xx	

c. Experimental results

Machine	Period 1	2	3	4
I	(1) 272.9	(2) 302.4	(4) 349.1	(7) 281.1
II	(2) 384.4	(3) 424.7	(5) 372.0	(1) 316.4
III	(3) 292.9	(4) 356.4	(6) 374.2	(2) 198.2
IV	(4) 530.2	(5) 425.2	(7) 309.9	(3) 464.7
V	(5) 304.1	(6) 510.6	(1) 412.2	(4) 417.4
VI	(6) 319.0	(7) 474.3	(2) 404.0	(5) 221.9
VII	(7) 457.8	(1) 337.9	(3) 327.1	(6) 403.4

Table II (continued)

d. Comparison table results

Treatment

vs.	(1)	(2)	(3)	(4)	(5)	(6)	(7)
(1)		+29.5 +68.0	+108.3 -10.8	+76.2 +5.2	+55.6 -108.1	+98.4 +65.5	+8.2 +119.9
(2)	-29.5 -68.0		+40.3 +94.7	+46.7 +158.2	-12.4 -182.1	+176.0 -85.0	-21.3 +70.3
(3)	-108.3 +10.8	-40.3 -94.7		+63.5 +65.5	-52.7 -39.5	+81.3 +76.3	-154.8 +130.7
(4)	-76.2 -5.2	-46.7 -158.2	-63.5 -65.5		-105.0 -113.3	+17.8 +93.2	-68.0 -220.3
(5)	-55.6 +108.1	+12.4 +182.1	+52.7 +39.5	+105.0 +113.3		+206.5 +97.1	-115.3 +252.4
(6)	-98.4 -65.5	-176.0 +85.0	-81.3 -76.3	-17.8 -93.2	-206.5 -97.1		+155.3 +54.4
(7)	-8.2 -119.9	+21.3 -70.3	+154.8 -130.7	+68.0 +220.3	+115.3 -252.4	-155.3 -54.4	
Sum	-515.9	-187.9	+62.2	+810.9	-998.2	+617.4	+211.5
Mn.	-36.8	-13.4	+4.4	+57.9	-71.3	+44.1	+15.1

meaning and no utility in change-over experiments. Accordingly, they are referred to more conveniently as Rows, from the point of view of writing out the Design.

In order to make the nature of the Design and the analysis abundantly clear, an actual experiment is reported. There were 7 ways of setting up Machines and 4 of the ways were tried on each of 7 Machines. This took 4 Periods of about 10 days on each Machine. The resulting yields are shown in Table IIc. Finally, in the comparisons of Table IId, it can be seen how the Treatments, or sets-up, for the Machines, compared within a given Machine. Thus Treatment (1) gave 29.5 lb. less than (2) on Machine I; by the same token (2) gave 29.5 lb. more than (1) on Machine I. Treatment (1) gave 68.0 lb. less than (2) on Machine II, etc. All in all Treatment (1) gave 515.9 lb. less than the Treatments (2) through (7) with which it occurred on the same Machines, I, II, V, and VII. (There is no question of comparing Treatment (1) to other Treatments on Machines where (1) was not tried.) Similar totals are formed for each Treatment (1) through (7). We must be struck by the fact that Treatment (4) did better than any other Treatment except (5) on all Machines on which it was tried and got a total of +810.9. From these totals averages may be struck (dividing by 14 and not 12 as one might have expected). From this we learn that Treatment (1) gave 36.8 lb. less on the average than all Treatments (including itself). It should nowise be supposed, of course, that the above extensive but primitive piece of arithmetic is intended as an example of how such a Youden rectangle should be analyzed in practice. This arithmetic is presented simply to justify and recommend the Design. Methods of analysis to be used in routine work are presented later; they are much less laborious and much surer methods.

It is also possible to try 3 Treatments on each of 7 Machines, when there are 7 Treatments to be tested, if one writes some Design, such as:

		Period	
Machine	1	2	3
I	(1)	(2)	(4)
II	(2)	(3)	(5)
III	(3)	(4)	(6)
IV	(4)	(5)	(7)
V	(5)	(6)	(1)
VI	(6)	(7)	(2)
VII	(7)	(1)	(3)

This Design preserves the virtue of Table Ia, that each Treatment occurs the same number of times on each Day, i.e., in each Column. Also if Machine I gives some production for Treatments, (1), (2) and (4), there are available the 3 comparisons (1) vs. (2), (1) vs. (4) and (2) vs. (4), free of the differences that can occur from Machine to Machine, i.e., within a given unit or Machine. The 21 such comparisons, from the present Design, just fill a comparison table like Table Ib. The Design gives rise to a once-filled table of comparisons. The above Design, with 7 Machines during 3 Periods can be called a Youden rectangle with 7 Treatments (t) in 3 Columns (c) for 7 Rows (r), or it may be called a 7x3x7 Youden. So far as actual use of the above 7x3x7 Design is concerned, when there are many experimental units, it may be repeated 3 times on 21 of them so that a good deal of information may be gotten conveniently.

It is impossible to write a Design, i.e., to try 7 Treatments on each unit 5 times and to satisfy the condition that all horizontal comparison, i.e., within Rows, should be made an equal number of times in 7 Rows. We might call this a 7x5x7 Design. Just where it is possible to write Youden rectangles is discussed in detail in Chap. III. How it is possible to write them is discussed in Chap. IV.

For general discussion of Youden rectangles the reader may refer to Youden (1951), to Cochran and Cox (1957), or to Fisher and Yates (1967, 27-31). The reader of the above cited works will find an elusive element of difficulty, if his practical experience is indeed of the change-over type. This difficulty is that the entire corpus of experimental Design rests on and reflects its origins in the field trials of Agronomy. The reader will find Youden rectangles are recommended for problems on, for instance, litters of animals where it is required to try about 19 Treatments on highly consistent litters of about 5. Such designs are recommended for laboratory and technological processes where there is a limited number of Treatments possible on a group and varietal trials where there is a large number of varieties. These recommendations are essentially in terms of field trials involving contiguous plots and seem a little odd if one is thinking essentially in terms of change-over experiments. Writers generally, within a classical type of experimental design, as in Agronomy, think of Youden rectangles as a means of getting smaller blocks and thus controlling variability. Sometimes, we are more interested in foreshortening the time required for an experiment. Even in change-over experiments, however, it may be argued that it is best to try only a few Treatments on a subject because over a short time he is presumably more consistent in his responses than he is over a long time. It can also be argued that insofar as subjects change variously with time, the short experiment is the better.

As was previously discussed, in connection with paired comparisons, each Youden rectangle is exactly paralleled by its balanced incomplete block. The distinction is that in Youdens the Column in which a symbol occurs is important, whereas it is not so in a balanced incomplete block. To put the matter otherwise, a Youden rectangle becomes a balanced incomplete block if the symbols within a Row are randomly permuted. The Design of Table IIa, for 7x4x7, thus becomes, for instance:

	Period			
Machine	1	2	3	4
I	(2)	(4)	(1)	(7)
II	(3)	(1)	(2)	(5)
III	(4)	(6)	(2)	(3)
IV	(4)	(3)	(5)	(7)
V	(6)	(5)	(4)	(1)
VI	(7)	(2)	(5)	(6)
VII	(6)	(1)	(3)	(7)

It will be noted that the Treatments on each Machine are exactly the same as in the corresponding Youden rectangle but they do not necessarily occur in the same Period. The Design obviously has not balanced Treatments against Periods. In the analysis the effects of only Machines and Treatments would be considered. If, of course, the Columns of a Youden rectangle prove, in some sense, unimportant, this class degenerates into balanced incomplete blocks. The same Design may be used but analyzed either as a Youden or as a balanced incomplete block. Note that any Youden Design can be used as balanced incomplete block Design but the reverse is not true.

Double Youdens - There exists an extension of the simple Youden rectangle, as first discussed, which may be termed the double Youden rectangle. It has most of the general properties of the well-known simple Youden rectangle. It has all Treatments equally represented, i.e., twice, in each Column.

The horizontal comparisons, i.e., within a Row, fill each cell in a comparison table an integer number of times. It differs only in that t Treatments require a Design of 2t Rows. In general it may be characterized as t x c x 2t . Such a Design could, of course, be used in experiments that are not change-over but are simply spacial, as in Agronomy. They have been so used by Cochran and Cox (1957) with the appellation of Type V. The necessity for such a Design arises because, as has previously been indicated, it is not always possible to write a Youden rectangle which might be convenient for an experimental program. Thus one cannot write a 7x5x7, i.e., the number of Treatments, t = 7 , the number of Columns, c = 5 , and the number of Rows, r = 7 , in a balanced Design. Thus in terms of a comparison table, like Table IIb, there would be 10 comparisons per Row or 70 in all to fill 21 cells of the comparison table (below the principal diagonal thereof). Accordingly, some would, at best, be filled thrice and some four times! The non-existence, for present purposes, of 7x5x7 is not important, in a practical way, because one can write 7x3x7, 7x4x7 and 7x6x7 (as will appear in the next Section). The issue does, however, become important if one has 9 Treatments because it is then impossible to write any Youden rectangles at all, except the special large case of 9x8x9 (Yates rectangle) in 8 Columns or the even larger latin square (9x9x9). This is very unfortunate because there always seems to be a lot of experiments with 9 Treatments. The practical difficulty can, however, be met by writing the double Youden rectangle 9x4x18, as in Table III. This only requires 4 Periods or Columns. Just when it is possible to write Youden rectangles or double Youdens can be discovered from Table IV which shows all possible Youden situations, single and multiple, for any number of Treatments

Table III Comparison of 9 Treatments in sets of 4

The 9x4x18 Double Youden

a. The Design by Groups and by Periods

Group	Period 1	2	3	4
I	(1)	(2)	(4)	(8)
II	(2)	(3)	(5)	(9)
III	(3)	(4)	(6)	(1)
IV	(4)	(5)	(7)	(2)
V	(5)	(6)	(8)	(3)
VI	(6)	(7)	(9)	(4)
VII	(7)	(8)	(1)	(5)
VIII	(8)	(9)	(2)	(6)
IX	(9)	(1)	(3)	(7)
X	(1)	(6)	(3)	(2)
XI	(2)	(7)	(4)	(3)
XII	(3)	(8)	(5)	(4)
XIII	(4)	(9)	(6)	(5)
XIV	(5)	(1)	(7)	(6)
XV	(6)	(2)	(8)	(7)
XVI	(7)	(3)	(9)	(8)
XVII	(8)	(4)	(1)	(9)
XVIII	(9)	(5)	(2)	(1)

b. The comparison table

	Treatment								
vs.	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
(1)		ooo	ooo	ooo	ooo	ooo	ooo	ooo	ooo
(2)	xyy		ooo	ooo	ooo	ooo	ooo	ooo	ooo
(3)	xxy	xyy		ooo	ooo	ooo	ooo	ooo	ooo
(4)	xxy	xxy	xyy		ooo	ooo	ooo	ooo	ooo
(5)	xyy	xxy	xxy	xyy		ooo	ooo	ooo	ooo
(6)	xyy	xyy	xxy	xxy	xyy		ooo	ooo	ooo
(7)	xxy	xyy	xyy	xxy	xxy	xyy		ooo	ooo
(8)	xxy	xxy	xyy	xyy	xxy	xxy	xyy		ooo
(9)	xyy	xxy	xxy	xyy	xyy	xxy	xxy	xyy	

up to $t = 36$ in any number $c < t - 1$ of Columns. As might be expected, the multiple Youdens have their parallel balanced incomplete blocks.

Table IIIa shows the Design for 9x4x18. Table IIb sets out the comparison table, i.e., the comparisons within Rows. Then we may check off the comparisons in the first 9 Rows with an x and in the last 9 Rows with a y . Thus Treatment (1) is compared with Treatment (2) once in the first half, i.e., in Row (Group) I and twice in the second half, i.e., in Rows X and XVIII. So its comparison shows xyy . The counter comparison of (2) with (1) may be again, as in Table I, indicated by an o. This comparison Table IIIb shows that the 9x4x18 gives rise to a thrice-filled table of comparisons. If the first half had been used alone some comparisons would have been made once and some twice and the same may be said of the second half. As things have been worked out, however, the two halves complement each other and all comparisons are made thrice.

These double Youdens may not only be useful when, for some value of t , like 9, there exists no single Youden Design, but also when they require a smaller number of Columns than the single Youden. Thus it is shown in Table IV that for 25 Treatments one can write the single Youdens 25x9x25 and 25x16x25 but it may nonetheless be more practical to employ the double Youden 25x4x50.

To go somewhat beyond the idea of double Youdens, it may be noted that there do exist various classes of multiple design where the number, $r = gt$, of Rows involves $g > 2$ but still an integer. Thus it might be said that the case of a triple Youden, 7x2x21, $f = 1$, has already been shown in Table I. While in a theoretical way we might get a variety of integers, it seems hardly worthwhile to consider cases other than $g = 2$, i.e., what the writer terms "double Youdens." The exception is the case of paired comparisons $c = 2$, where we may go to quite high multiples, or values of $g > 2$. Cochran and Cox (1957), it may be noted, do show multiple Designs beyond the double.

Yates rectangles - A special class of Designs was first suggested by Yates (1936), where r, the number of Rows, is the same as t, the number of Treatments, but c, the number of Columns is always $t - 1$. In these Designs every Treatment must, of course, occur in every Column. Furthermore, each Row must contain all Treatments but one and that one must be missing in no other Row. Yates rectangles may be conceived as simply writing down all combinations of t things $t - 1$ at a time, i.e., $C^{t}_{t-1} = t$ Rows, and then permuting within Rows until the condition that each Treatment occur once and only once in each Column be met. Such Designs can be written for any value of t. An example is the 7x6x7 Design as follows:

	Column					
Row	1	2	3	4	5	6
I	(1)	(2)	(4)	(7)	(6)	(3)
II	(2)	(3)	(5)	(1)	(7)	(4)
III	(3)	(4)	(6)	(2)	(1)	(5)
IV	(4)	(5)	(7)	(3)	(2)	(6)
V	(5)	(6)	(1)	(4)	(3)	(7)
VI	(6)	(7)	(2)	(5)	(4)	(1)
VII	(7)	(1)	(3)	(6)	(5)	(2)

The comparison table is always filled $t - 2$ times. This Design fills the comparison table 5-fold.

These rectangles due to Yates (1936) are of great interest historically because they anteceded Youden Designs in general. Yates had in mind not Change-over experiments but those more like Agronomic experiments where the observations were made on contiguous plots of land. These Yates rectangles are not of general utility because, among other things, they are overshadowed by the closely related latin squares which will be discussed next. There is, however, one exception and that is in problems involving Carry-over, which will be discussed below, and which makes latin squares with t odd quite awkward, whereas the associated Yates rectangle is very well behaved. It may be preferred to use the Yates rectangle.

Latin squares - A very special class of change-over Designs are those where the number of Rows and Columns is the same as the number of Treatments. These may be written t x t x t . They are called latin squares. In these, every Treatment must occur in every Row and in every Column. The comparison table is always filled t times. This well-known Design is abundantly illustrated in the remainder of the present book. The first case shown is that of Table VI.

Latin squares are of great general untility. They are comparatively easy to design. They are easy to analyze. Latin squares are of the greatest interest historically because they were among the things that Sir Ronald Fisher gave us when he inaugurated experimental design in the early 1920's. Originally, and in most of the current writing, latin squares arise typically as contiguous plots of land, rather than as Change-over experiments, but there seem to arise few difficulties on account of this background.

The historic order in which things arose was first the latin square, for practical purposes by Fisher in 1925, then the Yates (1936) rectangle, and then the Youden (1937) rectangle. Properly we should perhaps speak of Youden squares in contrast to latin squares, but that name has been so firmly established by the illustrious Sir Ronald Fisher, that it must be retained. To the contrary, in the literature Youden rectangles proper are often spoken of as incomplete latin squares. This term arises because the latin squares anteceded the Youdens. In a sense, this is a fair description because a Youden rectangle may always be completed to a latin square. On the other hand, it is nowise true that a latin square can always be reduced to a Youden rectangle. Commonly, it is quite impossible to choose any set of columns from the latin square and get a Youden rectangle. It is, of course, fair to call a Youden square, or latin square, complete in the sense that a given Row contains all Treatments once and once only. On the other hand, a Youden rectangle, $c < t$, is incomplete in the sense that a given Row contains only a fraction of the Treatments.

The usual textbook discussion of latin squares, arising as it does from spacial rather than Change-over experiments, discusses their character in terms best forgotten in the present context. They are said to possess a "double control," i.e., they are arranged so that systematic effects in the Rows and Columns do not affect the comparison of Treatment estimates. While this is true enough, it were best forgotten so far as change-over experiments are concerned. For them, just as for any other Youden ($c < t$) Design, Column effects are eliminated automatically from the Treatment comparisons because every Treatment occurs in each Column and their effects are quite unconfounded. The matter is best conceived as that of differences within Rows filling a comparison table t times.

It may be suggested in a general way that the latin square arises simply as a special case txtxt, of the Youden type txcxt, of Designs. It nonetheless has many special properties. It may be considered as an upper boundary of the Youden Designs in a very general sense. The latin square also is important since several rules as to existence of Youdens turn on it. These rules will not be discussed here further, however.

The reader will encounter in the literature much difficulty in the analysis of Youden rectangles and even more when he attempts to extend it to change-over experiments on account of the reverse order in which the Designs arose--from special case to general case. The literature attempts the form of the analysis of variance that Fisher derived for the latin square. With the development of the field, later writers have attempted to generalize the analysis of all types of design into the form of analysis of variance. This Procrustean effort is of dubious value particularly when one gets to the analysis of change-over experiments. The arithmetic becomes extremely heavy and the interpretation muddled. Accordingly, it seems best to recast the analysis of data from Youden rectangles in a form more natural, more meaningful and, in certain circumstances, more easily calculated. This form is, of course, applicable to latin squares for which it about ties with analysis of variance.

The latin square, like the previous Youden rectangles, has its degenerate form, when the character of the Columns is supposed trivial. This is known as randomized blocks. This Design is of considerable importance in laying out trials on plots of land but of no interest in Change-over experiments.

Metalatin Designs - We shall consider only very briefly cases where the number of Columns exceeds the number of Treatments, i.e., where $c > t$. Beyond latin squares, one may, at least theoretically, go on to cases where each experimental unit (Patient, Subject or Machine) try all Treatments and try some of them more than once. For instance, each subject might try all Treatments twice, in a rather simple-minded design. For such there seems to exist no name; therefore, let us coin the term of metalatin rectangles. There seem to be 3 types , Type III of Cochran and Cox (1957), where the number of Columns is $c = mt - 1 (m > 1)$; their Type IV where $c = mt + 1 (m > 0)$, m being an integer; and a nameless type which arises given $t \times c \times t$, c being such that $c' = c - t$ gives rise to a Youden Design, in the sense of the present book, $t \times c' \times t$. In these, some Treatment must occur more than once in a Row and therefore be compared within the Row with itself. In the comparison table the cells of the principal diagonal are to some extent filled--in the comparison table for $c \leq t$ these cells are empty. Approaching the problem from another direction, one will find such Designs discussed as latin squares with extra plots.

There is only one place where the use of metalatin Designs seems possibly justified, i.e., the case of $tx(t + 1)xt$. These are mentioned very favorably by Patterson and Lucas (1962). They favor a latin square where each Treatment is followed by every other Treatment and then by repeating the last Column so that it is also followed by itself. Such design makes Treatments and Carry-over completely orthogonal but at the expense of confounding with the Row. It is possible that some such Design would avoid the difficulties in Change-over referred to later, of writing latin squares with t odd , but the matter is not explored in the present book.

The Designs we have termed metalatin, i.e., of the present general type except that $c > t$, will not be discussed much further in this book so that perhaps a few general remarks are in order here. Metalatins may conceivably be useful in some unusual situations where one is poor in experimental units but comparatively rich in time. Such a case might, for instance, arise in a paper mill on an experiment on 3 great paper machines, which are all there are in a paper plant. Accordingly, one might try out 3 bactericides over more than 3 periods. The case is somewhat forced and the whole class seems so far outside all probable experience that it will not again be discussed beyond this section. The Design 3x5x3, taken from Cochran and Cox (1957, their plan 13.16 of their Type III), is

	Column				
Row	1	2	3	4	5
I	(1)	(2)	(3)	(2)	(3)
II	(2)	(3)	(1)	(1)	(2)
III	(3)	(1)	(2)	(3)	(1)

The resulting comparison table is

	Treatment		
vs.	(1)	(2)	(3)
(1)	xxoo	oooo oooo	oooo oooo
(2)	xxxx xxxx	xxoo	oooo oooo
(3)	xxxx xxxx	xxxx xxxx	xxoo

The historic limitations of experimental design - It so happened that the necessity for clear thinking with regard to experimental design and the analysis of experimental data were first realized in Agronomic work at Rothamsted Experimental Station. As a result the terminology of experimentation has an Agronomic flavor. In a most serious way, the whole realm of thinking is limited and stilted by the circumstances of Agronomy in the 1920's.

It should be understood that the agronomic experiment is peculiar in that in a general way it consists of units of area upon each of which, in a usual way, only one Treatment is tried. One tries only one fertilizer on a given piece of land. These small plots are usually clustered in comparatively homogeneous groups called blocks. There may be great variability from block to block although ideally but little within. The idea of the Block or of the Row, as we handle things, is very important. It is the dimension in which we accept chance variability. We anticipate there will be much more variability among Rows than within them. A contrast to Change-over, in behavioral work, that is analogous to the plots and blocks of Agronomy is what is often called matched panel operation. One Treatment is given to one group of men and another Treatment to another group. There are, say, two such groups. An effort is made to get men of about the same age, background, etc., matched or balanced into the 2 groups. Such endeavors generally seem to work out poorly. The experimental control of a change-over experiment seems to work out better.

The reader with a background in statistical experimentation will be surprised that the present book gives but little attention to the arrangement of Treatments, as compoundings of various factors etc., but great attention to the evaluation of Treatments regardless of their origin. The writer's experience has been that his great problem has been to discover the actual result of his Treatment--how people react to such and such a Treatment. How much steel did come from the use of each Oil? These facts require more than careful and abundant observation; they require the careful design with which this book is preeminently concerned; they require careful allowance for probable differences among the experimental units. The facts having been clearly discovered, the particular policy that should be pursued as a result of the experiment is often patent. There is, on occasion, necessity to break treatment variability up, as has been done in each of Chapters V through VIII in connection with various types of problem. This is essentially the partitioning of treatment effects that looms so large in the classical analysis of variance. Indeed, Table XXXVI is devoted to such partitioning. It will, however, be noted that the numerous examples of the present book are almost entirely concerned with the question, in fact, of what was the response to various Treatments in simultaneous trials.

Finally, experimental practice and analysis are still heavily influenced by the fact that they were directed towards the use of desk calculators. A science carries its rudimentary aspects in the same way that animals carry rudimentary organs. One can indeed still find in statistical texts elements from the pen-and-paper days preceding the desk calculator early in this century. In fact, we have the electronic computer and should shape our work for its use. The matter of form of analysis will be taken up fully in the later chapters.

II. Exploration of Youden field

Page

The general character of Youden Designs 32

Latin squares and Yates rectangles 34

Youden Designs, $c \lesssim t - 2$, that may exist 34

Single Changes-over or paired comparisons 36

The general character of Youden Designs - For the type of Design indicated in Chapter I, we may write in a formal way that it involves t Treatments, applied to c Columns in $r = gt$ (g an integer) Rows. For $c \leq t$, no Treatment is repeated within a Row, whence each Treatment occurs g times in each Column. In order that the comparison table, in the sense of Chapter I, be filled an integer number of times, the number,

$$2rC_2^c = rc(c - 1) \tag{1}$$

of comparisons within a Row over all Rows must be a whole multiple of the total,

$$2C_2^t = t(t - 1) \tag{2}$$

number of combinations in the comparison table, i.e., the schedule of paired comparisons. The integer fill is then

$$\begin{aligned} f &= rC_2^c/C_2^t \\ &= rc(c - 1)/t(t - 1) \quad . \end{aligned} \tag{3}$$

The value r is generally chosen so that f is minimal. The case of $r = t$ is, of course, that of the usual (single) Youden. Then, of course, we get the simplification

$$f = c(c - 1)/(t - 1) \quad . \tag{4}$$

Cases where $r = gt$, g an integer, greater than 1, but minimal, are what we have termed multiple Youdens. We are particularly interested in the case of $g = 2$, which is here termed a double Youden.

The term Youden is used in the present book in the somewhat restricted sense of Designs that seem to have proven useful and easily useful. They are restricted to situations where the numbers of Rows, typically subjects, and corresponding to blocks of the classical experiment in Agronomy, is equal to the number of Treatments. There is also a special class where the number of Rows is twice the number of Treatments. Each Treatment appears the same number of times (once or twice, according to type of design) in each Column. Very importantly, each Treatment is compared with each other Treatment the same number of times within some Row. The number of comparisons within Rows is called the fill. The term Youden is used somewhat more broadly than usual in the literature, to include not only the common cases where the number of Columns is less than the number of Treatments but also cases where Columns equal Treatments.

There exist, in the literature, Designs where the number of Rows is less than t, whence a given Treatment cannot be represented in every Column. There are also Designs where the number of Rows is greater than the number of Treatments, but not an integer multiple. In the terms of the immediately preceding discussion, g is not an integer. Such Designs are unhandy to execute and put an unnecessary strain on one's faith in the algebraic interpretation. It seems idle to get into such unnecessary complications. It were better to spend our time understanding fully, and familiarizing ourselves with, the more limited type of Designs, covered in the present book. The present book excludes the metalatins, where $c > t$.

Latin squares and Yates rectangles - It is always possible to write a latin square, which requires t Rows. Then $g = 1$. Then from Equ. (4), since $c = t$, $f = t$. Similarly, it is always possible to write a Yates rectangle, which requires t Rows and $c = t - 1$, whence from Equ. (4), $f = t - 2$.

Youden Designs, $c \leq t - 2$, that may exist - In order to find what Designs are possible, for a given number t of Treatments, one need only consider the value of f in Equ. (4) for any value of c (with that t). Then a single Youden can exist for cases where f is an integer. Double Youdens are cases where f comes out .5, 1.5, 2.5, etc., and then it is necessary to make $g = 2$. Triple Youdens are cases where f comes out a multiple of 1/3 and then it is necessary to make $g = 3$. Using thus Equ. (4), Table IV was constructed. This table shows for t through 36 all single and double Youdens than can possibly exist and for t through 16 all triple Youdens. For each Design, the fill, f, in the appropriate comparison table is shown. It should, of course, be realized that the conditions considered in Table IV are necessary but not sufficient. Only for the cases shown can there exist a Youden rectangle, but not for all cases has a Design been written.

It will be noted in Table IV that if any Design, single, double or triple, for t Treatments can be written in c Columns, then for the same t Treatments, one can be written in $t - c$ Columns. One may term two such Designs complementary. The procedure for so doing is discussed in Fisher and Yates (1967:26).

Table IV - Possible Youdenish arrangements with given number t of Treatments, c of Columns , resulting in fill of f

a. Youdens single or proper (excluding Yates rectangles and latin squares), all possible up to t = 36

t	c	f	t	c	f	t	c	f	t	c	f	t	c	f
7	3	1	16	6	2	23	11	5	31	6	1	35	17	8
	4	2		10	6		12	6		10	3		18	9
										15	7			
11	5	2	19	9	4	25	9	3		16	8	36	15	6
	6	3		10	5		16	10		21	14		21	12
13	4	1	21	5	1	27	13	6		25	20			
	9	6		16	12		14	7						
									34	12	4			
15	7	3	22	7	2*	29	8	2*		22	14			
	8	4		15	10*		21	15*						

b. Double Youdens, all possible up to t = 36

t	c	f	t	c	f	t	c	f	t	c	f	t	c	f
5	2	1	13	3	1	21	6	3	25	4	1	29	7	3
	3	3		6	5		10	9		12	11		14	13
				7	7		11	11		13	13		15	15
9	4	3		10	15		15	21		21	35		22	33
	5	5												
			17	8	7							33	16	15
				9	9								17	17

c. Triple Youdens, all possible up to t = 16

t	c	f	t	c	f	t	c	f	t	c	f	t	c	f
4	2	2	7	2	1	10	3	2	13	5	5	16	5	4
				5	10		4	4		8	14		11	22
							6	10						
							7	14						

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* Proved non-existent.

One can only suppose that the possible Designs of Table IV would soon be obvious to anyone who looked at all closely at the present field. Such a list is given, over somewhat different range from that of Table IV, by Fisher and Yates (1967). They show the cases for which it has been proven that no Design is possible. Often, however, the reader may be told the Designs that are known but not those possible. In this sense, some are missing even from Cochran and Cox (1957). They show in their list of plans all the single Youdens up to the case of $t = 21$ and occasional higher values of t. They omit Designs that exist for $t = 23$ and 31; that possibly exist for $t = 27$, 34, 35 and 36; and that might be expected but are proven not to exist for $t = 22$ and 29. A number of other writers give lists but these seem to refer back pretty generally to Cochran and Cox. Many more Designs are added in the present book, but even here Designs remain unfound.

It is probably surprising that when $t \leq 36$ how few single Youdens are even possible from Table IVa. There t is most commonly odd, because then it is much easier to satisfy Equ. (4). There are even fewer double Youdens because for them it is almost necessary that t be some multiple of 4 plus 1. Such limitations are not, however, important in practice. For instance, with 10 Treatments to test, one could be tried twice, or some other 11th could be found, and so the very convenient Youdens of 11x5x11 or 11x6x11 would become applicable.

Single Changes-over or paired comparisons - Paired comparisons, i.e., cases where only 2 Treatments are offered in succession to each participant or machine initiated the entire present discussion. They remain of peculiar practical importance. Only in their case are we interested in Designs where g is larger, and sometimes considerably larger, than 2. Then, of course, it is always possible to write a Design. For t Treatments it is only necessary to write $r = C_2^t$ Rows to get all comparisons. This indeed does for t , odd, although if it is also required that each Treatment occur equally in each of the 2 Columns it is necessary to write $r = 2C_2^t$ Rows. Under these circumstances, paired comparisons are always filled once for t , odd, and twice for t , even. Chapter VIII is devoted to the consideration of such paired comparisons and includes some discussion of their design.

III. The Carry-over of Treatment effects

Page

The general problem of Carry-over in Change-over experiments 39

Carry-over in a simple non-Youden situation 41

Change-over and Carry-over in a latin square 46

Carry-over in Youden Designs (c < t) 50

Carry-over in paired comparisons 53

Contiguity problems 54

The general problem of Carry-over in Change-over experiments - Change-over experiments while admirable in many respects suffer from the problem of the Carry-over of effects from one period of Treatment into at least the following period under another Treatment. For example, in the classical field of Animal Husbandry where a cow's lactation may be subdivided into three Periods each under a different diet, the effects, such as vitamin A content, of one Period must persist into the following Period. Likewise, in an industrial experiment on the use of bactericide in the recirculated water of a paper machine, the bactericide of the 2nd period may persist into the 3rd period. A similar problem arises when people are given a series of Treatments. The character of one Treatment tried, for example, in the 2nd week, may influence their response to Treatment tried in the 3rd week. The matter may be one of simple physical persistence of Treatment or it may be a matter of changed attitude on the part of a patient. It is often easy enough to show experimentally that such Carry-over occur without being able to judge whether the effect is physical or psychic. While such problems seem to have been first considered in feeding problems of Animal Husbandry, they occur, however, widely and clearly in studies made in many fields. The carry-over effect present is usually boldly ignored, often to the total confusion of the experimental results.

The possibility of Carry-over is very important because it influences not only the analysis of data from change-over experiments but their design. Occasionally one wants information on Carry-over *per se* -- how long does a Treatment effect persist? More commonly one wants to free the estimates of direct Treatment effects from Carry-over with which they may be confounded.

Even the latter is not always done. It may be sufficient to balance carry-over so that it affects all Treatments equally. Certainly it is folly to have a given Treatment often or even always preceded by some other Treatment which may cast a heavy shadow. Yet the literature is full of such designs. The balancing of carry-over effects can, moreover, be usually done at no experimental or monetary cost -- it is simply a matter of arrangement of Treatment sequences.

The general field of Change-over experiments has been reviewed by Patterson and Lucas (1962). Their review allows for what seems to the writer extremely complicated Design; it allows for cases where say a number of subjects are first divided into subgroups which are comparatively homogeneous within themselves and on all this is the Design built. The writer thinks that perhaps such complexity has unduly burdened the application of their techniques. Certainly the writer has had trouble enough to get the more simple of their Designs, as discussed in the present book into use. Even then comparatively simple Designs have suffered from men disappearing or at least failing to complete their assignment.

In the previous discussion the idea of Carry-over effects from an immediately preceding Period is involved rather than, say, from some yet earlier Period. Indeed the following discussion is almost entirely in terms of the immediately preceding Period. In fact, very commonly one can find traces of Carry-over for 2 Periods. It usually seems to be much the same as the Carry-over of immediately preceding Period but weaker. Theoretically, and to some extent practically, one may be plagued by the Carry-over extending back several weeks in an experiment.

Carry-over in a simple non-Youden situation - A clear illustration of Carry-over comes from a very simple experiment for which data and Design are shown in Table Va. Treatment (1), which, as will appear, proved inferior, was tried against regular Treatment (2). This Design does involve Change-over but is outside any of our regular Youdenish patterns. It is presented because the Design was such that the evidence of Carry-over is unequivocal. Each Treatment was tried by each of a number of men. Some men started with (1) and some with (2) and then followed a sequence of trials. The men were divided into 12 groups each of about 20 men. The men of a given group followed a given sequence of trials. For each Week there was calculated the percentage of the time that the men said they were satisfied with the Treatment.

The 0th week was set aside, as a conditioning week, i.e., as a week in which a participant tried one or the other of the two competitive Treatments but he returned no comment (at least one that was used). For the next 4 weeks he was given one Treatment to try for each week, and his satisfaction was considered. It can be seen that in each case there is an (1) preceded by a (2) and one preceded by an (1); similarly there is a (2) preceded by a (2) and one preceded by an (1). The symbols (1) and (2) are balanced in Weeks. It is, accordingly, interesting to summarize the results according to the Treatment given and the Treatment preceding as shown in Table Vb. Finally, as in Vc averages are formed. The things that are striking about these numbers are that Treatment (1) averages less than Treatment (2) but secondly that both Treatments average higher after (2) than after (1). That is to say that the character of the two Treatments is evidenced just about as well in the response to other Treatment a week later as it is evidenced in the week that the Treatment is tried. The carry-over effect is of much the same size and nature as the direct or main effect. Our evidence seems to suggest that the use of good Treatment improves performance of subsequent Treatment in much the same way as an experience with bad Treatment depresses performance of subsequent Treatment.

It may be noted that the Design of Table Va is perfect for the strict detection of both the direct effect of the two Treatments and for their Carry-over. As the statistical term is they are not confounded with column or week effects. Each Treatment is tried equally each Week and each Change-over and presumably Carry-over occurs equally each Week of the trial.

Carry-over may be physical as when an animal is subject to a succession of diets and traces of diet in one Period may linger on into the next Period. Again it could be physical if medication similarly lingered. In this case we should call the Carry-over positive because benevolent Treatment tends to give a desirable response in the following Period. On the other hand it could be psychological. Someone judging the degree of satisfaction of a Treatment may be influenced by the satisfaction of some previous Treatment. Then it is hard to say whether the Carry-over will be positive or negative. If the preceding Treatment was satisfactory then the Treatment under immediate consideration may be judged favorably in association in which case we say there is a positive Carry-over. Here good Treatment tends to elicit favorable response at the next Period. On the other hand, if the preceding Treatment was satisfactory then that under

Table V - Two Treatments each tried by 12 Groups of Men

a. Design and Results

	Week					
Group	0	1	2	3	4	Sum
I	((2))	(1) 72.3	(1) 56.9	(2) 59.4	(2) 70.0	258.6
II	((1))	(2) 76.0	(2) 75.0	(1) 72.8	(1) 77.5	301.3
III	((2))	(1) 61.7	(1) 51.7	(2) 61.7	(2) 64.2	239.3
IV	((1))	(2) 75.6	(2) 75.6	(1) 71.1	(1) 70.2	292.5
V	((2))	(1) 36.4	(1) 38.2	(2) 52.7	(2) 57.8	185.1
VI	((1))	(2) 61.8	(2) 49.1	(1) 61.8	(1) 70.9	243.6
VII	((2))	(1) 65.5	(1) 56.4	(2) 61.8	(2) 81.8	265.5
VIII	((1))	(2) 57.8	(2) 70.6	(1) 60.0	(1) 62.0	250.4
IX	((2))	(1) 60.0	(1) 62.7	(2) 68.2	(2) 69.7	260.6
X	((1))	(2) 68.9	(2) 67.8	(1) 60.0	(1) 57.8	254.5
XI	((2))	(1) 63.8	(1) 58.3	(2) 60.7	(2) 67.8	250.6
XII	((1))	(2) 60.4	(2) 67.3	(1) 67.3	(1) 54.4	249.4
Sum		760.2	729.6	757.5	804.1	3051.4
Mean		63.4	60.8	63.1	67.0	63.6

b. Sorting results according to Treatment and previous Treatment

	Treatment				
After	(1)		(2)		Sum
(1)	56.9	56.4	59.4	61.8	
	77.5	62.0	76.0	57.8	
	51.7	62.7	61.7	68.2	
	70.2	57.8	75.6	68.9	
	38.2	58.3	52.7	60.7	
	70.9	54.4 = 717.0	61.8	60.4 = 765.0	1482.0
(2)	72.3	65.5	70.0	81.8	
	72.8	60.0	75.0	70.6	
	61.7	60.0	64.2	69.7	
	71.1	60.0	75.6	67.8	
	36.4	63.8	57.8	67.8	
	61.8	67.3 = 752.7	49.1	67.3 = 816.7	1569.4
Sum		1469.7		1581.7	3051.4

c. Averages

	Treatment			
After	(1)	(2)	Mean	Contrib.
(1)	59.75	63.75	61.75	- 1.82
(2)	62.72	68.06	65.39	+ 1.82
Mean	61.24	65.90	63.57	
Contrib.	-2.33	+2.33		

immediate consideration may be judged harshly in contrast, in which case we say there is negative Carry-over. It is indeed hard to tell how matters of judgment will go and one can only be guided by the ascertained facts.

Insofar as the reader is impressed by these facts and by others of the same kind produced later, he should be warned of a curious phenomenon. The irresistible inclination of people, who are not statistical, is to start discussing the machinery of such an effect. They want to argue as to what must go on in the minds of the participants. That seems an innocent enough thing to consider. The next thing, however, they become so involved in one another's hypothetical machinery that they start disputing about it. They proceed tangentially to the question of the machinery of how Carry-over might work. There arises much speculation as to what is in the hearts and the minds of the participants. They end up by proving to their own satisfaction that some machinery does not exist and feel that they have therefore dismissed the fact. The fact remains actually. Accordingly, we should not discuss the fairly obvious machinery but stick to the facts and their implications. Our proper business is to discover, in fact, whether (and not why) the response of participants to some type of Treatment is affected by previous Treatment.

Men who pass as practical are inclined to confuse themselves with the foregoing type of result or to ignore it entirely. They are inclined to try 2 Treatments, one after the other. The first Week they may try one Treatment entirely, the second Week the other. By doing this they get a situation where the effect of particular Weeks is confounded together with carry-over effects with true treatment effects. They had better almost guessed at the quality of their Treatment. At a somewhat more sophisticated level they may have half the men try a given Treatment in one Week and the other half the men try the other Treatment. Then the second Week each man will try the Treatment he was previously denied. There are, however, dangers in such a 2-treatment experiment completed in 2 Periods. Where Treatment effects are significant we see what would tend to have happened if we had run 2-treatment studies, of the type where half the Groups get Treatment (1) the first Week and Treatment (2) the second Week, while the other half get (2) the first Week and (1) the second Week. This is what may be characterized as the straightforward, commonsense approach to the question. We must, however, ask ourselves how far such a commonsense program wisely advises and how far it misleads. It can be seen that the typical 2-week, 2-treatment study tends to underestimate in this case the extent to which the Treatments differ from the control; it judges Treatment (1) which probably deviates considerably from (2) as deviating little. They may judge poor product, (1) i.e., (63%), as in Table V when preceded by good Treatment, (2), to be much the same as good Treatment, (2), i.e., (64%), when preceded by poor Treatment (1). Would we then do right to say that the two kinds of Treatment are the same?

Given the data of Table V, a well-instructed man would have two serious alternatives in reporting or recommending. First, he might say that there had been tried out the two Treatments properly to balance out any possible carry-over effects so that at least they are equally associated with the treatment effects, in the Design above. Then (1) preceded equally by (1) and (2) gives 61% satisfaction whereas (2) preceded equally by (1) and (2) gives 66% satisfaction. As an alternative, he might report that a man using one kind of Treatment at all regularly will be more like (2) after (2), i.e., 68%, and (1) after (1), i.e., 60%.

If there is Carry-over but it is of negative character, i.e., makes a man judge particularly ill Treatment in general after he has experienced good Treatment, problems of quite another kind would arise. The alternation of Treatments (1) and (2) would tend to heighten the contrast between them and greatly gratify an experimenter anxious to find statistical significance of some kind. He might report that Treatment (1) gave very low satisfaction while (2) gave very high satisfaction. He, in this way, might mislead. If the experience of having tried (2) much depressed the satisfaction enjoyed with further such then (2) would appear to much less advantage in the long run than on the short run of the experiment.

Change-over and Carry-over in a latin square - The realm where the writer and it seems other people have done the most work and thinking on Change-over and where we have the most experience on Carry-over is that of the latin square with t , the number of Treatments, even. For cases of t , even, there is always possible, as is discussed in Chapter IV, a Design with the desirable kind of Change-over, as below. Consider, in illustration, a test on 6 Treatments, (1) through (6), for which results, in a measure of satisfaction, were as shown in Table VIa. The Design is a regular latin square (except for the conditioning Week marked 0 which is extra) but it has the unusual feature that each Treatment follows all other Treatments so that we may form some opinions about the Carry-over. These data may be sorted, along the same lines as the data of Table V, according to Treatment and preceding

Treatment. This has been done in Table VIb. It will be seen that, in general, if a Treatment has a high average the results following that Treatment are high, i.e., the means for Rows and Columns in VIb go together. In such experiments with several Treatments tried, when the Treatments have considerable effect the Carry-over is considerable--it is very often in the same direction or so-to-speak positive. It may be noted in this case we get again the curious result that poor Treatment following good Treatment does as well or better than good Treatment following poor Treatment. So there can be a serious danger of misleading by bad experimental design and analysis. In Table VIb the original Columns are equally represented in each Treatment (vertically) and following each Treatment (horizontally). The original Rows remain equally represented in each Treatment but, of course, the original Rows are a little confounded with the Carries-over. Thus in row I, the Carry-over of Treatment (1) appears twice but the Carry-over of Treatment (4), not at all.

In Table VIb, there is shown against each Carry-over the mean. The departure of the mean for Treatment or for Carry-over (After) from the corner mean of 56.4 is shown as the appropriate contribution. It is of some interest to notice the correspondence between Treatment and carry-over effects. This can be assessed by calculating the correlation coefficient* for means or for contributions. It is considerable, being +.93.

Typically in the kind of experiment involved in Table VIa, the conditioning or 0th week would be omitted. Accordingly, the values on the

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*In case anyone does not know, the correlation coefficient measures the correspondence of two sets of figures. It is +1.00 for perfect direct correspondence; .00 for no correspondence and -1.00 for perfect contrariwise correspondence.

Table VI - Satisfaction reported by 6 Groups on 6 Treatments over 6 Weeks

a. Data collected

Group	0	Week 1	2	3	4	5	6	Sum	Mean
I	((1))	(1)46.4	(3)45.8	(2)40.8	(5)62.4	(6)59.9	(4)61.7	317.0	52.8
II	((2))	(2)60.9	(4)59.2	(3)44.9	(6)64.2	(1)55.3	(5)52.0	336.5	56.1
III	((3))	(3)50.0	(5)50.0	(4)64.2	(1)60.9	(2)58.4	(6)53.3	336.8	56.1
IV	((4))	(4)63.7	(6)72.0	(5)71.7	(2)57.3	(3)53.3	(1)52.7	370.7	61.8
V	((5))	(5)48.8	(1)50.4	(6)56.4	(3)58.9	(4)65.6	(2)64.2	344.3	57.4
VI	((6))	(6)63.2	(2)58.7	(1)51.8	(4)49.6	(5)49.6	(3)53.6	326.5	54.4
Sum		333.0	336.1	329.8	353.3	342.1	337.5	2031.8	
Mean		55.5	56.0	55.0	58.9	57.0	56.2		56.4

b. Data arranged by Treatment and by Treatment of the preceding Week

After	Treatment (1)	(2)	(3)	(4)	(5)	(6)	Sum	Mean	Contrib.
(1)	46.4	58.4	45.8	49.6	52.0	56.4	308.6	51.4	-5.0
(2)	51.8	60.9	53.3	59.2	62.4	53.3	340.9	56.8	+ .4
(3)	52.7	40.8	50.0	65.6	50.0	64.2	323.3	53.9	-2.5
(4)	60.9	64.2	44.9	63.7	49.6	72.0	355.3	59.2	+2.8
(5)	50.4	57.3	53.6	64.2	48.8	59.9	334.2	55.7	- .7
(6)	55.3	58.7	58.9	61.7	71.7	63.2	369.5	61.6	+5.2
Sum	317.5	340.3	306.5	364.0	334.5	369.0	2031.8		
Mean	52.9	56.7	51.1	60.7	55.8	61.5		56.4	
Contrib.	-3.5	+.3	-5.3	+4.3	-.6	+5.1			

principal diagonal of Table VIb would be omitted, i.e., the results for a given Treatment would not include a case where the Treatment would be preceded by itself, i.e., the treatment total would not embrace its own Carry-over. The mean for that Treatment would be necessarily biased by the imbalance of the Carries-over. The corresponding estimate of Carry-over would be necessarily biased in the direction of the balance of the Treatments. The Carry-over, in brief, as will be discussed in detail in later discussion on Carry-over, would,to some extent, be confounded with direct treatment effects as well as previously discussed, to some extent with row effects. This confounding is, however, small in degree in a latin square, as compared with other Designs. The direct treatment effects would be confounded to some extent with Carry-over but with nought else.

Since in many cases the Carry-over of a Treatment varies as much, or almost as much, as the direct effect of the Treatment, it might in some sense be used to discover the Treatment effect. Perhaps even more pointfully the direct Treatment effect and Carry-over might be used conjointly to discover the effect of Treatment. Such procedure would be very efficient but it is perhaps a shade questionable under the practical conditions. It has not been developed.

The occurrence of Carry-over in routine latin squares, particularly of the type 4x4x4 for t even and a kind of double Design two (3x3x3) for t odd, as will be discussed later, has been thoroughly examined by the writer in numerous tests using men. The conclusions reached have been that the pattern of Treatment of participants should be such that one test Treatment has much the same background as another. In practice, the Designs, termed balanced Carry-over, should be regularly employed. This should be done because there is often clearly a Carry-over of Treatment responses from the Treatment used prior to a given test Treatment. Insofar as the preceding point is sound, experiments consisting of several (such as 6 kinds of Treatment) are superior to those consisting of 2 kinds of Treatment. The experiment on 2 Treatments or Materials in 2 Periods, to which the uninstructed mind will be found to turn freely may be bad because possible Carry-over can be balanced only with a certain difficulty as in Table V. In experiments with $t > 2$ one does not even need the conditioning Period as shown at the beginning in Table VI; this matter will be gone into later in detailed discussion of the analysis of experimental data. The conditioning 0th Week of Table VI is simply included because for the moment it simplifies the argument as to the existence of Carry-over. In practice Designs with $t=2$ should be used as little as possible.

The idea of confounding of effects will occur frequently throughout the following discussion and so should perhaps be considered now. It occurs in Table VI in the sense that the Carry-over of Treatment (1) appears twice in Group I and not at all in Group IV. Now if for some reason these Groups are inclined to give higher readings than the other Groups, the Carry-over of Treatment (1) will appear favorable. There is nothing but good luck to distinguish such possible Group effects from effects of Carry-over. They are confounded.

Carry-over in Youden Designs ($c < t$) - In Youden Designs, other than the latin squares, where the number of Columns is less than the number of Treatments, ($c < t$), it is difficult to discover the general nature of Carry-over in so simple a way as from a latin square. There the confounding of Carry-over with Rows is trivial, so that the Carry-over stands out, as in Table VIb, pretty plain. When, however, $c < t$ as in Table II ($c = 4$, $t = 7$) , even the calculation of direct Treatment effects in the Youden is not immediately obvious. Certain Treatments and certain Changes-over occur in some Rows but not in others and if those happen to be Rows with high values the Treatments or Changes-over, involved, appear high at first glance. When $c < t$, it is difficult, or impossible, to demonstrate Carry-over in any rough but at all convincing way. It does occur and can be demonstrated by more involved analysis, as will be done in Chapter VI. With Youdens, $c < t$, it is possible, but generally impracticable, to use a conditioning Period such as that shown for the latin square of Table VI.

In case, Carry-over effects do occur in Youdens, $c < t$, they can be controlled to a large extent; we may at least reassure ourselves that no Treatment is repeatedly preceded by some other Treatment so that the direct effect of the first is confounded with the Carry-over of the second. In a Design such as that of Table IIa, 7x4x7, plainly we cannot have Treatment (2) preceded by every other Treatment because (2) occurs but four times. The most we can ask is that (2) or any other given Treatment, shall not be preceded by a given other Treatment more than once. This was achieved in the Design presented although the matter was not pointed out at the time. There are two rewards for this arrangement. First, if we neglect Carry-over, any possible tendency to obscure main Treatment effects will be minimized. Secondly, if we choose we can set up the necessary equations to solve for Carry-over--they will be considerable although, of course, far from the almost impossible level that they would attain if we just randomly assigned Treatments to Groups. If we suppose that in the first Column, there was common background Carry-over, the pattern of direct Treatment and Carry-over is as follows:

	Treatment						
After	(1)	(2)	(3)	(4)	(5)	(6)	(7)
(1)		x	x	x			
(2)			x	x	x		
(3)				x	x	x	
(4)					x	x	x
(5)	x					x	x
(6)	x	x					x
(7)	x	x	x				
Common	x	x	x	x	x	x	x

It can be seen that the appearance of both types of effect, in themselves, are regular and also in conjunction with each other are regular. Thus is the isolation of the effects simplified and strengthened. All the Youden squares used in the present book are constructed when possible on this principle of no repetition of Carry-over. It has been necessary to rewrite the Designs given by the textbooks to achieve this. By so doing, not only is the effect of Carry-over minimized but a good basis has been laid to estimate it by algebraic operation, if that be required.

The desirability, or one might even say necessity, of controlling Carry-over in experimental design, bears on the question of balanced incomplete blocks, as discussed in Chapter I, in connection with Table II. In the little example given there of a balanced incomplete block gotten by random permutation within the Rows of a 7x4x7 Youden, we find, if we ignore the first Column, which has perhaps some common background Carry-over, the pattern of direct Treatment and Carry-over as follows:

Treatment

After	(1)	(2)	(3)	(4)	(5)	(6)	(7)
(1)		x	x				x
(2)			x	x	xx		
(3)	x				x		x
(4)	xx		x			x	
(5)				x		x	x
(6)	x	x			x		
(7)		x					

It can be seen that the Carry-over is irregular. Thus it is plain that the disposition of Treatments in Columns may be of some importance even in balanced incomplete blocks. This necessity bears on design of the type discussed in this book, where one must constantly wonder whether it is necessary to control the occurrence of Treatments according to Column, i.e., whether Youden Designs are really necessary or whether balanced incomplete blocks might not suffice. Such a question is inevitable if effect of Columns seems, as it often does, trivial. The difficulty remains, however, that when the procedure is by Change-over, Carry-over will be confounded with Treatment. The problem we are faced with is that even if Columns are not important in themselves, the Change-over may amount to the same thing. So the balanced incomplete block cannot actually be employed freely and one might as well use Youden Design anyhow. Then in the analysis the effect of Columns can be ignored but Carry-over can still be considered.

Carry-over in paired comparisons - In paired comparisons, i.e., Changes-over involving 2 Treatments per unit, carry-over effects may occur just as in any of the Designs discussed just previously. Carry-over may be simply allowed for by controlling the Treatments that a given Treatment follows or it may be estimated. An example of unrepeated Change-over is given in Table I where it can be seen that no given Treatment is preceded twice by any other Treatment. Shortly after that table there is given an example of testing

4 Treatments where the Design has not only unrepeated Change-over but balanced Change-over. A given Treatment is preceded by all other Treatments. It may be noted that if it is required to have a given Treatment preceded by all other Treatments it is necessary to write $2C_2^t$ Rows for t, odd, the same as for t, even. A conditioning 0th Period may be used or simply a preceding Period of common background. The former is illustrated later in Table XXXVIII although it is hard to work such things out. The latter (common background) is the more practical.

Contiguity problems - The previous discussion of Change-over where one designs in 1-space of a Row and time progresses to the right is closely related to the even more general and not uncommon problem of contiguity of Treatments, in 2-space. This is somewhat outside the purpose of the present discussion but may be touched on briefly here. Thus even in the classic field of Agronomy a Treatment may effect the response to another contiguous to it. Agronomy is no proper part of our business, so we must leave this discussion as a suggestion. One must wonder, however, whether in the classical agronomic experiments contiguity may not have some bearing, whether it has not been neglected and whether it should not be controlled. The writer has discussed with Dr. W. J. Youden his work in this realm but there seems to be no extensive literature on the subject.

The type of Design, discussed in the present work, that assures that no Treatment is ever preceded twice by any other given Treatment automatically assures us also that no given Treatment follows it twice. Hence no Treatment has any other Treatment beside it unduly. For work as in Agronomy, however, it would be necessary to consider neighbors in a second dimension. By way of a suggestion, an appropriate Design, with unrepeated Change-over in Rows and from Row to Row, can be written in the case of spacial latin squares. The matter is discussed at more length in Chapter IV, in connection with the topic of latin squares, t even. Here we may simply give the example of a 6-treatment experiment, like that shown in Table VI, but with somewhat different arrangement, i.e.,

(1)	(3)	(2)	(5)	(6)	(4)
(3)	(5)	(4)	(1)	(2)	(6)
(2)	(4)	(3)	(6)	(1)	(5)
(5)	(1)	(6)	(3)	(4)	(2)
(6)	(2)	(1)	(4)	(5)	(3)
(4)	(6)	(5)	(2)	(3)	(1)

This is gotten, of course, by simply rearranging the Rows of Table VI. Here, of course, contiguity in both Rows and Columns is well taken care of. Any Treatment like (1) has any other Treatment (2) through (5) contiguous in either a Row or Column 4 times. It may be of concern that 20/36 of the plots are edge plots. If so there might be put buffer plots, judiciously treated but not counted, about the Design as above. Perhaps we might designate buffer plots by double parenthesization and write:

	((1))	((3))	((2))	((5))	((6))	((4))	
((1))	(1)	(3)	(2)	(5)	(6)	(4)	((4))
((3))	(3)	(5)	(4)	(1)	(2)	(6)	((6))
((2))	(2)	(4)	(3)	(6)	(1)	(5)	((5))
((5))	(5)	(1)	(6)	(3)	(4)	(2)	((2))
((6))	(6)	(2)	(1)	(4)	(5)	(3)	((3))
((4))	(4)	(6)	(5)	(2)	(3)	(1)	((1))
	((4))	((6))	((5))	((2))	((3))	((1))	

Now any Treatment like (1) has every Treatment, including itself, contiguous in either a Row or a Column 4 times.

Two positions, with regard to analysis, might be taken with such data, just as they may be taken in handling change-over experiments. The more simple position is to comfort oneself that insofar as contiguity is of importance, no Treatment will be heavily confounded by the nature of its neighbors. The more difficult position is to eliminate, if necessary, the effect of contiguity from the estimates of effect of Treatment and always eliminate it from the estimates of residual variability. Such analysis could be easily enough completed along lines indicated in the later chapters. Contiguity, presumably, would work not only in two dimensions but so-to-speak, backwards and forwards.

IV. Writing Youden Designs

	Page
The general problem of writing Youden designs	58
Latin squares, t even	65
Paired latin squares, 2(txtxt), t odd	73
Yates rectangles, tx(t - 1)xt	75
Single Youdens where $c = (t \pm 1)/2$	79
Single Youdens where $f = 1$	80
Intractable single Youdens	84
Paired Youdens for balance of Change-over	89
Double Youdens where $c = (t \pm 1)/2$	92
Double Youdens, non-central	96
Single Change-over	97

The general problem of writing Youden designs - It should be realized that while Table II shows what Youden rectangles, txcxr, where r = t or = 2t, can exist, it does not necessarily prove they do exist. In fact the literature knows of two, as discussed later, that do not exist and a third such case has been discovered, as in connection with Table XIV, in the course of the present work. For some no Design of any kind has ever been submitted, as was discussed in Chapter II, on exploration of the Youden field, and even in this book no Design is submitted. Even granting the existence of a Design in a general way for any particular case it may not exist subject to the restriction that no given Treatment is ever preceded (or for that matter followed) by any other Treatment more than once.

It seems good practice to write Designs, indicated as possible in Table II, subject to the restriction that the Treatments in a Column are arranged in cyclic order. This matter was illustrated in the Designs of 7x3x7 and 7x4x7 which were previously presented and discussed at some length. To repeat the Design for the latter:

	Column			
Row	1	2	3	4
I	(1)	(2)	(4)	(7)
II	(2)	(3)	(5)	(1)
III	(3)	(4)	(6)	(2)
IV	(4)	(5)	(7)	(3)
V	(5)	(6)	(1)	(4)
VI	(6)	(7)	(2)	(5)
VII	(7)	(1)	(3)	(6)

The advantages of such Design, when it is possible, are many. In the first place one may examine the forward differences in Row I, i.e., 1, 2 and 3.

These containing no repetition, it follows that Treatment (1) can never be followed twice by any other Treatment. From the relationship existing between any two Rows, it follows that if Treatment (1) is never followed twice by any other Treatment that the same kind of statement can be made for (2), (3)...(7). There are many theoretical advantages such as the fact that the sequence of forward differences 1, 2 and 3 proves immediately that the Design is Youden but on these let us not dwell. Rather let us consider the enormous practical advantage of being able to hand such simple Design to the unsophisticated man who must apply it in the fury of the clinic. To its simplicity he can comply--let the reader examine some of the noncyclic examples that follow and imagine himself applying them in either fury or heat. Further let us remember the ease with which we can discover a misapplication of a Design in the present simple form; it can be done at a rapid glance; it requires no elaborate proofing. The writer suspects that one of the reasons that adequate experimental design has found as little application as it seems to have found, is that it often seems too complex to administer in practical situations.

Cyclic form is not only easy to proof and easy to manipulate but it is easy to abbreviate. For instance, we may abbreviate the case of 7x4x7, just shown to

7x4x7		f = 2		
Row I	(1)	(2)	(4)	(7)
Δ		1	2	3

when it is understood that the Columns are, of course, written in cyclic order, modulo t . First forward differences, which may be useful, have been added to the statement of Treatments in first Row. The fill, which may be useful, has been noted against each Design. The other example, previously employed, can be written much more cunningly and economically as

7x3x7	f = 1		
Row I	(1)	(2)	(4)
Δ		1	2

This form saves a great deal of space. The forward differences, added, reassure us that the Change-over is indeed unrepeated. They are also some reassurance against copying errors. Double Youdens can be written in similar abbreviated form. Thus we can write 5x2x10, f = 1 , at length as:

	Column	
	1	2
Row I	(1)	(2)
II	(2)	(3)
III	(2)	(4)
IV	(4)	(5)
V	(5)	(1)
VI	(1)	(4)
VII	(2)	(5)
VIII	(3)	(1)
IX	(4)	(2)
X	(5)	(3)

or in abbreviated form we may write

5x2x10	f = 1	
Row I	(1)	(2)
Δ		1
VI	(1)	(4)
Δ		3

The few multiple Youdens, $g > 2$ when $c = 2$ with which we are concerned, may be written in the abbreviated form:

4x2x12		f = 2
Row I	(1)	(2)
Δ		1
V	(1)	(3)
Δ		2
IX	(1)	(4)
Δ		3

The Designs, just shown for 7x3x7 and 7x4x7 are cases where no Change-over is repeated but they are not balanced in the sense that every Treatment is preceded by every other Treatment. There are simply not enough Columns. They are cases of the type that was recommended in Chap. III as the best possible for distinguishing Carry-over from direct treatment effect. In Designs with unrepeated Change-over at least no Treatment is ever preceded (or for that matter followed) more than once by any other Treatment. The Designs with balanced Change-over have every Treatment preceded once and once only by all other Treatments. Let us turn at once to an example of the latter, i.e., the double Youden, 9x5x18, $f = 5$, as in Table VII. It can be seen there where every Treatment following (6) is, for the sake of illustration, underlined, that (6) is followed by everything (except of course, (6)). It is inevitable that no single Youden $c < t$ can be balanced. There are $t - 2$ or fewer Changes-over so no Treatment can be followed by all $t - 1$ other Treatments. Designs with balanced Change-over seem very desirable. If one is working roughly one may comfort oneself by ignoring any possible Carry-over saying that it will have little or no effect on the judgment of treatment effects. If one is working more exactly it is comparatively easy to eliminate arithmetically, or to estimate arithmetically, the magnitude of such Carry-over. This matter is discussed later in the section on analysis of results. Looking at the matter in another way, unrepeated Change-over gives the maximum chance of separating Carry-over from direct treatment effects.

For latin squares, it is possible to balance Change-over in an especial way by having them preceded by a 0^{th} conditioning Period (or Column), as in Table VI. Such a conditioning Period is possible for single

Table VII. A double Youden 9x5x18, f=5, illustrating balanced Change-over.

Row	Column 1	2	3	4	5
I	(1)	(2)	(4)	(7)	(3)
II	(2)	(3)	(5)	(8)	(4)
III	(3)	(4)	(6)	(9)	(5)
IV	(4)	(5)	(7)	(1)	(6)
V	(5)	(6)	(8)	(2)	(7)
VI	(6)	(7)	(9)	(3)	(8)
VII	(7)	(8)	(1)	(4)	(9)
VIII	(8)	(9)	(2)	(5)	(1)
IX	(9)	(1)	(3)	(6)	(2)
X	(1)	(9)	(6)	(4)	(8)
XI	(2)	(1)	(7)	(5)	(9)
XII	(3)	(2)	(8)	(6)	(1)
XIII	(4)	(3)	(9)	(7)	(2)
XIV	(5)	(4)	(1)	(8)	(3)
XV	(6)	(5)	(2)	(9)	(4)
XVI	(7)	(6)	(3)	(1)	(5)
XVII	(8)	(7)	(4)	(2)	(6)
XVIII	(9)	(8)	(5)	(3)	(7)

or multiple Youdens, but is unprofitable since balance of this especial character remains unachieved.

The following sections list various single Youden Designs, $t \times c \times t$, $c \leq t$, for purposes of listing subdivided into kinds essentially classified according to the ease of their writing but also bearing some correspondence to their utility. The range of Designs covered in these tables is, arbitrarily, $2 \leqslant t \leqslant 36$ and $2 \leqslant c \leqslant 18$. Over this range Designs are shown wherever possible. There are commonly many more Designs for a given $t \times c \times r$ situation than the one shown in the following tables. In some cases it is impossible, with exhaustive search, to find a Design, and this is reported. Finally, there are some Designs which are still unformed for lack of time.

The Designs shown in the following tables were found to some extent in the literature--essentially from Cochran and Cox (1957) who give an extensive table of actual Designs of single Youdens of the type to which we have restricted ourselves. They make little distinction between cyclic and non-cyclic Designs although some are of each type. Also, some of their non-cyclic Designs can easily be put in cyclic order. An extensive table of balanced incomplete blocks is given by Fisher and Yates (1957). Such is the correspondence between these and Youdens that some Youdens can be extracted. They point out that some Designs can be written in cyclic form. They do not allude to the utility of this feature; Cox (1958) stresses the usefulness of cyclic Designs. None of these sources was anywise concerned about Change-over or any other form of contiguity. One finds in the literature, indeed, many Designs where a given Treatment is preceded in all cases by some other one Treatment, so that a given Carry-over must be completely confounded with some given Treatment effect. All the Designs that could be adopted required rearrangement, particularly with regard to unrepeated Change-over. It was necessary indeed to work out most of the following Designs. They were found principally by various devices which will not be discussed here* because they would surely not forward the business of this book which is to facilitate the application of such Designs.

It should perhaps be mentioned, in a cautionary way, that the experimental Designs to be indicated shortly, and all through the present book are not to be subjected to any randomization. This has to be mentioned because randomization is so commonly recommended that some innocent may take it for granted. Randomization, beyond perhaps the ordering of the experimental units (Men, Machines or whatever) would be of no use in the Designs proposed. As was discussed in Chapter III, randomization within the Rows of the present Designs will turn them into balanced incomplete blocks which will probably be appropriate if one is doing agronomic work but will be highly inappropriate for change-over experiments. Some people may be uneasy at the set and systematic nature of our Designs. They may be concerned that successive experiments may, in some way, be correlated. Such uneasiness may be allayed by their assigning Treatment numbers randomly.

The Youden Designs $c < t$ recommended themselves to us, as at the beginning of this book, because we could get work done quickly. By employing many participants one could form opinions on t Treatments in less than t Periods. There is, of course, a limit to such economy because, from Equ. (4), if $f \geq 1$,

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*Beall, G. & J. J. Ferris. On discovering Youden rectangles with Columns of Treatments in cyclic order. Research Bulletin 71-37. Princeton, N. J.: Educational Testing Service, 1971.

$$c \geq \sqrt{t} \tag{5}$$

for single Youdens. Similarly, for double Youdens,

$$c \geq \sqrt{t/2} \quad . \tag{6}$$

It should be noted that in the present change-over Youden rectangles and latin squares, as in such Designs more generally, each Treatment occurs once, and once only, in each Period. Thus are the effects of Treatment freed from any systematic additive effect of Periods. In the literature, particularly of Animal Husbandry, one may find much concern as to whether such effects of Treatment are systematic. Thus it may be asked what is the pattern of lactation of a cow with time. Such enquiry may be very important but is in the present book avoided; here the concentration is on the restricted question of what does Treatment do. The larger question of investigating the entire system requires a familiarity with more general statistical theory.

Latin squares, t even - It is desirable to have latin squares $t \times t \times t$, t even, with the usual property of each Treatment once and once only in each Row and in each Column. For present purposes they should additionally have Treatments in Columns in cyclic order (mod. t). They should also have unrepeated Change-over, i.e., no Treatment to be preceded by any other Treatment more than once. For a latin square the last condition actually means automatically balanced Change-over, i.e., each Treatment to be preceded by all other Treatments. To indicate the nature of a Design it is, of course, sufficient, as discussed previously, to write the first Row. For that matter, in order to set up the Design, it is sufficient to investigate the first Row. Thus if one wants to write a latin square of the present type for 4 Treatments, i.e., 4x4x4 with fill, f = 4 , a suitable Design is

Row	Column 1	2	3	4
I	(1)	(2)	(4)	(3)
II	(2)	(3)	(1)	(4)
III	(3)	(4)	(2)	(1)
IV	(4)	(1)	(3)	(2)

It is, however, sufficient to write

4x4x4				f = 4
Row I	(1)	(2)	(4)	(3)
Δ		1	2	3

Bradley (1958) has provided a method for designing latin squares with unrepeated Change-over, for t even. His specifications for filling the first Row are as follows. Assign successively the integers from 1 to t to the t cells in the first Row by proceeding from left to right entering only cells in odd-numbered Columns, then reversing direction fill cells in even-numbered Columns. Then complete the Columns in cyclic order, modulo t . Thus for 8x8x8 one gets the Design,

Row I	(1)	(8)	(2)	(7)	(3)	(6)	(4)	(5)
Δ		7	2	5	4	3	6	1

As he observes, while the Period immediately preceding Treatment t can be occupied but once by any other Treatment, the one before that is occupied by only two Treatments. Specifically, for Treatment (k) these are Treatments (k - 1) and (k + 1) . This is somewhat undesirable if there is any tendency towards 2-period Carry-over.

It seems worthwhile to write Designs free, as far as possible, from this shortcoming in the period preceding, but one, a given Treatment. Such designs for latin squares, are shown in Table VIII. Every Treatment is followed once and once only by every other Treatment. This is accomplished by writing the Treatments in the first Row so that no forward difference is repeated. Designs of this kind have been found for all cases of t ,even,that have been at all extensively examined. There is only one solution for $t = 2$, two solutions for $t = 4$, while there are four solutions for $t = 6$ and increasingly large numbers of solutions as t increases. In Table VIII, there is shown only one Design for each value of t . It was chosen so that Periods previous to that immediately antecedent contained as little as possible the same Treatments. In the Designs given for 6x6x6, 10x10x10, 12x12x12, 16x16x16 and 18x18x18 no Treatment is repeated two Periods previous, nor for that matter, three or four or whatever previous. It will be noted that the numbers 6, 10, etc., are all one less than a prime number. The quality of the Designs seems to be related to the matter discussed in connection with Table IX. For 8x8x8, there exists, by exhaustive examination, no Design where the Treatment two weeks previous is unrepeated, so that the Design of Table VIII is the best that can be done. It is improbable that there is such a settlement for 14x14x14 so that given in Table VIII is as good as possible. These solutions are not difficult to find* even as t becomes great, so that Table VIII could be much extended if anyone wanted larger latin squares of this type.

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*Beall, G. On writing latin squares with unrepeated Change-over. Educational Testing Service, Princeton, N. J. (in process 1971).

Table VIII - Designs for latin squares, t even, with Columns cyclic

2x2x2 f = 2*

Row I	(1)	(2)
Δ		1

4x4x4 f = 4

I	(1)	(2)	(4)	(3)
Δ		1	2	3

6x6x6 f = 6

I	(1)	(3)	(2)	(5)	(6)	(4)
Δ		2	5	3	1	4

8x8x8 f = 8

Row I	(1)	(2)	(4)	(7)	(3)	(8)	(6)	(5)
Δ		1	2	3	4	5	6	7

10x10x10 f = 10

I	(1)	(2)	(9)	(3)	(5)	(10)	(8)	(4)	(7)	(6)
Δ		1	7	4	2	5	8	6	3	9

12x12x12 f = 12

Row I	(1)	(2)	(5)	(3)	(10)	(6)	(12)	(4)	(9)	(11)	(8)	(7)
Δ		1	3	10	7	8	6	4	5	2	9	11

14x14x14 f = 14

Row I	(1)	(4)	(3)	(5)	(9)	(14)	(6)	(13)	(7)	(2)	(12)	(10)	(11)	(8)
Δ		3	13	2	4	5	6	7	8	9	10	12	1	11

16x16x16 f = 16

Row I	(1)	(2)	(4)	(7)	(11)	(16)	(6)	(13)	(5)	(14)	(8)	(3)	(15)	(12)	(10)	(9)
Δ		1	2	3	4	5	6	7	8	9	10	11	12	13	14	15

18x18x18 f = 18

Row I	(1)	(2)	(14)	(3)	(17)	(15)	(7)	(4)	(9)	(18)	(13)	(16)	(6)	(8)	(12)	(5)	(11)	(10)
Δ		1	12	7	14	16	10	15	5	9	13	3	8	2	4	11	6	17

32x32x32 f = 32

Row I	(1)	(2)	(4)	(7)	(11)	(16)	(22)	(29)	(5)	(14)	(24)	(3)	(15)	(28)	(10)	(25)	(9)
Δ		1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16

contin.	Row I	(26)	(12)	(31)	(19)	(8)	(30)	(21)	(13)	(6)	(32)	(27)	(23)	(20)	(18)	(17)
contin.	Δ	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31

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*For the Design of 2x2x2, no satisfactory analysis can be made with estimation of both direct treatment effects and Carry-over. The matter is discussed at length in Chap. V.

The latin squares where t is a power of 2 are particularly easy to write in cyclic column form. Thus we have for 4x4x4 the Design as in Table VIII and one other solution. For t = 8 , i.e., 8x8x8 the solution as shown and other solutions. For 16x16x16 one can write, as can be seen, a Design with forward differences of 1, 2, 3, etc. through 15. For 32x32x32 one can again write this kind of Design, where the forward differences stand in arithmetic series. All this is convenient, in at least a small way, because Designs involving the powers of 2 are very popular in some circles and one may accordingly easily be called upon to evaluate 2^n kinds of treatments. For Designs of this kind there sometimes appears a given Treatment twice at the Period two weeks previous to another given Treatment but this may be somewhat better than the proposal of Bradley (1958), as previously.

For squares with t + 1 prime there is a second type of Design where the Columns are not in cyclic order but which is worthy of mention nonetheless. It is illustrated in Table IX. It involves constant forward differences in a given Row, modulo t + 1 , where t is the order of the square and different forward differences in each Row. Thus for 4x4x4, the forward differences in the first Row are 1, in the second Row 2 etc. The Designs of the type in Table IX do have the advantage that they can be very rapidly found, whereas those of Table VIII require a little searching. So if one wanted to go beyond the latter, it is fairly obvious how Designs might be written for t = 22, 28, 30 etc. In the Designs of Table IX no Treatment is repeated two Periods previous to a given Treatment, nor for that matter, three or four or whatever previous. A similar result is obtained by Alimena (1962), again for t + 1 prime, but by what seem more involved and difficult methods. He refers to the Designs as "perfectly counterbalanced latin squares."

Table IX - Typical Designs for latin squares, t + 1 prime

4x4x4 f = 4

	Column			
Row	1	2	3	4
I	(1)	(2)	(3)	(4)
II	(2)	(4)	(1)	(3)
III	(3)	(1)	(4)	(2)
IV	(4)	(3)	(2)	(1)

6x6x6 f = 6

	Column					
Row	1	2	3	4	5	6
I	(1)	(2)	(3)	(4)	(5)	(6)
II	(2)	(4)	(6)	(1)	(3)	(5)
III	(3)	(6)	(2)	(5)	(1)	(4)
IV	(4)	(1)	(5)	(2)	(6)	(3)
V	(5)	(3)	(1)	(6)	(4)	(2)
VI	(6)	(5)	(4)	(3)	(2)	(1)

10x10x10 f = 10

	Column									
Row	1	2	3	4	5	6	7	8	9	10
I	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
II	(2)	(4)	(6)	(8)	(10)	(1)	(3)	(5)	(7)	(9)
III	(3)	(6)	(9)	(1)	(4)	(7)	(10)	(2)	(5)	(8)
IV	(4)	(8)	(1)	(5)	(9)	(2)	(6)	(10)	(3)	(7)
V	(5)	(10)	(4)	(9)	(3)	(8)	(2)	(7)	(1)	(6)
VI	(6)	(1)	(7)	(2)	(8)	(3)	(9)	(4)	(10)	(5)
VII	(7)	(3)	(10)	(6)	(2)	(9)	(5)	(1)	(8)	(4)
VIII	(8)	(5)	(2)	(10)	(7)	(4)	(1)	(9)	(6)	(3)
IX	(9)	(7)	(5)	(3)	(1)	(10)	(8)	(6)	(4)	(2)
X	(10)	(9)	(8)	(7)	(6)	(5)	(4)	(3)	(2)	(1)

It will, of course, be realized that the Designs of Table IX are quite difficult to assign to the unsophisticated person, previously invoked, who may have to apply tnem in the clinic. He will probably botch somewhere and we shall be lucky if we notice that. If possible, give him a cyclic Design.

The difference in type from Table VIII of Table IX is, however, not substantial, because the Designs of that table may be transmuted into cyclic-column form. Thus in the case of the 4x4x4, if one call Treatment (4), Treatment (3) and vice versa and similarly interchange the names of rows III and IV, it becomes the

Row I	(1)	(2)	(4)	(3)
Δ		1	2	3

of Table X. By the interchanges

(2) → (3)
(3) → (2)
(4) → (5)
(5) → (6)
(6) → (4)

and appropriate Row renaming the 6x6x6 of Table IX becomes

Row I	(1)	(3)	(2)	(5)	(6)	(4)
Δ		2	5	3	1	4

The 10x10x10 is very easily so transmuted. Obviously, these cases with t + 1 prime when the background of previous Treatments is totally unrepeated will give rise by such transmutation into cyclic Designs with the same quality. Such Designs were mentioned in connection with Table VIII.

The Designs of Table IX have contiguity balanced in 2-space. This has been previously indicated in Chap. II, which introduced the matter of Carry-over. On the other hand, those of Table VIII have contiguity confounded with Treatment in 2-space. It is, however, also possible to control

2-dimensional contiguity in the Designs from Table VIII, by rearranging Rows. One is governed by the order in which the Treatments appear in the first Row and must rearrange the Rows so that the Treatments in the first Column appear in the same order. Thus given the 6x6x6, one might write

Row	Δ		2	5	3	1	4
I		(1)	(3)	(2)	(5)	(6)	(4)
II	2	(3)	(5)	(4)	(1)	(2)	(6)
III	5	(2)	(4)	(3)	(6)	(1)	(5)
IV	3	(5)	(2)	(6)	(3)	(4)	(2)
V	1	(6)	(3)	(1)	(4)	(5)	(3)
VI	4	(4)	(1)	(5)	(2)	(3)	(1)

Such working from Table VIII makes Designs for $t + 1$, not prime, possible, whereas Table IX suffers from that limitation.

In setting up latin squares it is always theoretically possible, generally desirable but commonly impolitic for $t > 2$ to precede the general Design by a conditioning Column (or Period). This is conceived as a Period when various Treatments are administered or Materials are tried, according to appropriate plan, in order to introduce as conveniently as possible the Carry-over effects that may be in the experiment. The analysis will be discussed later. From the point of view of designing the matter is very simple--the program that could otherwise be the first Column is administered beforehand. In the illustrative Designs below, the indication is the double-bracketed Treatments are for conditioning, i.e., for the sake of subsequent Carry-over. No experimental results would be recorded for these Periods. Thus for the two kinds of 6x6x6 Designs one might alternatively write:

Column

Row	0	1	2	3	4	5	6
I	((1))	(1)	(2)	(6)	(3)	(5)	(4)
II	((2))	(2)	(3)	(1)	(4)	(6)	(5)
III	((3))	(3)	(4)	(2)	(5)	(1)	(6)
IV	((4))	(4)	(5)	(3)	(6)	(2)	(1)
V	((5))	(5)	(6)	(4)	(1)	(3)	(2)
VI	((6))	(6)	(1)	(5)	(2)	(4)	(3)

or

Column

Row	0	1	2	3	4	5	6
I	((1))	(1)	(2)	(3)	(4)	(5)	(6)
II	((2))	(2)	(4)	(6)	(1)	(3)	(5)
III	((3))	(3)	(6)	(2)	(5)	(1)	(4)
IV	((4))	(4)	(1)	(5)	(2)	(6)	(3)
V	((5))	(5)	(3)	(1)	(6)	(4)	(2)
VI	((6))	(6)	(5)	(4)	(3)	(2)	(1)

Paired latin squares, 2(txtxt), t odd - Designs for paired latin squares, 2(txtxt), are shown in Table X. It is necessary to write these later squares in this form because it is impossible to use the more simple form of txtxt, with Columns cyclic and no repetition of Change-over, as when t was even. In the case of 5x5x5, for instance, it is, of course, easy enough to write a latin square with cyclic Columns and all latin squares are Youden. It is, however, impossible to avoid repetition of Change-over, so that in the sense of the present discussion, it is impossible to write a Youden Design. If one thinks of writing Designs in the form of Table VIII, i.e., of the first Row of treatment numbers and the forward differences of those treatment numbers, then it is impossible to write the Treatments from (1) through (t) in any order such that at least one forward difference will not be repeated. The theory of the matter has been discussed by Houston (1966). It is possible, however, to write 2 such latin squares where the forward difference repeated in one is omitted in the other and vice versa.

Table X - Designs for paired latin squares, 2(txtxt) with cyclic Columns and balanced Change-over.

2(3x3x3) f=6

Row I	(1)	(2)	(3)
△		1	1
IV	(1)	(3)	(2)
△		2	2

2(5x5x5) f=10

I	(1)	(2)	(4)	(3)	(5)
△		1	2	4	2
VI	(1)	(5)	(3)	(4)	(2)
△		4	3	1	3

2(7x7x7) f=14

I	(1)	(2)	(4)	(7)	(6)	(3)	(5)
△		1	2	3	6	4	2
VIII	(1)	(7)	(5)	(2)	(3)	(6)	(4)
△		6	5	4	1	3	5

2(9x9x9) f=18

Row I	(1)	(2)	(5)	(9)	(7)	(4)	(3)	(8)	(6)
△		1	3	4	7	6	8	5	7
X	(1)	(9)	(6)	(2)	(4)	(7)	(8)	(3)	(5)
△		8	6	5	2	3	1	4	2

2(11x11x11) f=22

I	(1)	(9)	(3)	(10)	(2)	(4)	(8)	(7)	(5)	(6)	(11)
△		8	5	7	3	2	4	10	9	1	5
XII	(1)	(4)	(10)	(3)	(11)	(9)	(5)	(6)	(8)	(7)	(2)
△		3	6	4	8	9	7	1	2	10	6

2(13x13x13) f=26

Row I	(1)	(2)	(4)	(7)	(11)	(6)	(12)	(8)	(13)	(10)	(9)	(3)	(5)
△		1	2	3	4	8	6	9	5	10	12	7	2
XIV	(1)	(13)	(11)	(8)	(4)	(9)	(3)	(7)	(2)	(5)	(6)	(12)	(10)
△		12	11	10	9	5	7	4	8	3	1	6	11

2(15x15x15) f=30

Row I	(1)	(2)	(4)	(7)	(11)	(6)	(12)	(8)	(15)	(9)	(14)	(13)	(10)	(3)	(5)
△		1	2	3	4	10	6	11	7	9	5	14	12	8	2
XVI	(1)	(15)	(13)	(10)	(6)	(11)	(5)	(9)	(2)	(8)	(3)	(4)	(7)	(14)	(12)
△		14	13	12	11	5	9	4	8	6	10	1	3	7	13

2(17x17x17) f=34

Row I	(1)	(2)	(4)	(7)	(11)	(16)	(6)	(14)	(8)	(17)	(13)	(12)	(9)	(15)	(10)	(3)	(5)
△		1	2	3	4	5	7	8	11	9	13	16	14	6	12	10	2
XVIII	(1)	(17)	(15)	(12)	(8)	(3)	(13)	(5)	(11)	(2)	(6)	(7)	(10)	(4)	(9)	(16)	(14)
△		16	15	14	13	12	10	9	6	8	4	1	3	11	5	7	15

2(19x19x19) f=38

Row I	(1)	(2)	(4)	(7)	(11)	(16)	(6)	(12)	(19)	(15)	(9)	(17)	(14)	(13)	(8)	(18)	(10)	(3)	(5)
△		1	2	3	4	5	9	6	7	15	13	8	16	18	14	10	11	12	2
XX	(1)	(19)	(17)	(14)	(10)	(5)	(15)	(9)	(2)	(6)	(12)	(4)	(7)	(8)	(13)	(3)	(11)	(18)	(16)
△		18	17	16	15	14	10	13	12	4	6	11	3	1	5	9	8	7	17

Accordingly, one gets a total Design where every Change-over occurs twice. A list of such Designs is shown in Table X. The claims made for these Designs are illustrated in Table XI for the case of 2(5x5x5), $f = 10$. It is hardly necessary to draw up the comparison table because obviously any such Treatment as (1) is compared once with every other Treatment in each Row and there are 10 such Rows. The question of what Treatments follow what may be of some interest and so the matter is tabled.

For the paired latin squares, as previously for the single latin squares, t even, it is probable that one might introduce a preliminary conditioning Period or Column for the sake of Carry-over.

Yates rectangles, $t \times (t - 1) \times t$ - Designs for Yates rectangles $t \times (t - 1) \times t$, t even or odd, are shown in Table XII. It is possible always to write Yates rectangles with Treatments in cyclic order in Columns and with unrepeated Change-over. Their presentation in Table XII is similar to that of the latin squares in Table VIII, i.e., the Treatments of the 1st Row and their forward differences are shown.

As has been previously pointed out in connection with the Design of Youdens, generally speaking when $t \leq 36$, $c < t - 1$, is even one gets few single Youden arrangements and no double, whereas for t odd, and of the same magnitude, $c < t - 1$, there are many single and double Youdens. In the related field of balanced Carry-over latin squares $c = t$, the situation is in a sense opposite; the squares with t even can be written with Change-over well-balanced but the squares with t odd cannot be written with satisfactory Change-over; we have to use the device of paired latin squares, as discussed previously. The happiest class of designs are the Yates rectangles, $c = t - 1$, which can always be written just as we want them.

Table XI - The character of paired latin squares illustrated in the case of 2(5x5x5), f=10

a. Design, as from Table X, in full

	Column				
Row	1	2	3	4	5
I	(1)	(2)	(4)	(3)	(5)
II	(2)	(3)	(5)	(4)	(1)
III	(3)	(4)	(1)	(5)	(2)
IV	(4)	(5)	(2)	(1)	(3)
V	(5)	(1)	(3)	(2)	(4)
VI	(1)	(5)	(3)	(4)	(2)
VII	(2)	(1)	(4)	(5)	(3)
VIII	(3)	(2)	(5)	(1)	(4)
IX	(4)	(3)	(1)	(2)	(5)
X	(5)	(4)	(2)	(3)	(1)

	Treatment				
After	(1)	(2)	(3)	(4)	(5)
(1)		xx	xx	xx	xx
(2)	xx		xx	xx	xx
(3)	xx	xx		xx	xx
(4)	xx	xx	xx		xx
(5)	xx	xx	xx	xx	
Bk.Gr.	xx	xx	xx	xx	xx

Table XII. Designs for Yates rectangles.

3x2x3 f=1*

Row	I	(1)	(2)
	Δ		1

4x3x4 f=2**

	I	(1)	(3)	(4)
	Δ		2	1

5x4x5 f=3

	I	(1)	(5)	(3)	(4)
	Δ		4	3	1

6x5x6 f=4

	I	(1)	(3)	(2)	(5)	(6)
	Δ		2	5	3	1

7x6x7 f=5

Row	I	(1)	(2)	(4)	(7)	(6)	(3)
	Δ	-	1	2	3	6	4

8x7x8 f=6

Row	I	(1)	(2)	(4)	(7)	(3)	(8)	(6)
	Δ		1	2	3	4	5	6

9x8x9 f=7

Row	I	(1)	(2)	(5)	(9)	(7)	(4)	(3)	(8)
	Δ		1	3	4	7	6	8	5

10x9x10 f=8

Row	I	(1)	(2)	(9)	(3)	(5)	(10)	(3)	(4)	(7)
	Δ		1	7	4	2	5	3	6	3

11x10x11 f= 9

	I	(1)	(2)	(5)	(10)	(6)	(4)	(3)	(9)	(11)	(8)
	Δ		1	3	5	7	9	10	6	2	8

12x11x12 f=11

Row	I	(1)	(2)	(5)	(3)	(10)	(6)	(12)	(4)	(9)	(11)	(8)
	Δ		1	3	10	7	8	6	4	5	2	9

13x12x13 f=11

Row	I	(1)	(2)	(4)	(7)	(11)	(6)	(12)	(8)	(13)	(10)	(9)	(3)
	Δ		1	2	3	4	8	6	9	5	10	12	7

14x13x14 f=12

Row	I	(1)	(4)	(3)	(5)	(9)	(14)	(6)	(13)	(7)	(2)	(12)	(10)	(11)
	Δ		3	13	2	4	5	6	7	8	9	10	12	1

15x14x15 f=13.

Row	I	(1)	(2)	(4)	(7)	(11)	(6)	(12)	(8)	(15)	(9)	(14)	(13)	(10)	(3)
	Δ		1	2	3	4	10	6	11	7	9	5	14	12	8

16x15x16 f=14

Row	I	(1)	(2)	(4)	(7)	(11)	(16)	(6)	(13)	(5)	(14)	(8)	(3)	(15)	(12)	(10)
	Δ		1	2	3	4	5	6	7	8	9	10	11	12	13	14

16x15x16 f=14

Row I	(1)	(7)	(14)	(13)	(2)	(4)	(16)	(3)	(11)	(8)	(12)	(10)	(5)	(6)	(15)
Δ		6	7	15	5	2	12	3	8	13	4	14	11	1	9

17x15x17 f=15

I	(1)	(2)	(4)	(7)	(11)	(16)	(6)	(14)	(8)	(17)	(13)	(12)	(9)	(15)	(10)	(3)
Δ		1	2	3	4	5	7	8	11	9	13	16	14	6	12	10

18x17x18 f = 16

Row I	(1)	(2)	(14)	(3)	(17)	(15)	(7)	(4)	(9)	(18)	(13)	(16)	(6)	(8)	(12)	(5)	(11)
Δ		1	12	7	14	16	10	15	5	9	13	3	8	2	4	11	6

19x18x19 f=17

ow I	(1)	(2)	(4)	(7)	(11)	(16)	(6)	(12)	(19)	(15)	(9)	(17)	(14)	(13)	(8)	(18)	(10)	(3)
Δ		1	2	3	4	5	9	6	7	15	13	8	16	18	14	10	11	12

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*The Design, 3x2x3 cannot be used to estimate Carry-over. It is advisable to use the paired Design

Row I	(1)	(2)
Δ		1
IV	(1)	(3)
Δ		2

See Table XXXII for example.

**The Design, 4x3x4; will yield estimates of direct Treatment and Carry-over but provide no test of significance without repetition.

They are of particular importance in the case of t odd, for which the paired latin squares are necessary, if latin squares one would have in the present connection. Considerable difficulty arises, as in connection with Table X with this. Most of the advantages of the latin square can be obtained from a Yates rectangle with less difficulty in design and application. In a sense the Yates rectangle may play the role of the latin square when t is odd and might profitably be used far more frequently than seems the case.

From the comparison of Tables VIII and XII, plainly for t , even, the Yates is gotten by dropping the last Column of a latin square and this approach may be usefully extended to Designs of the type shown in Table IX. Thus if you should want the Youden, 22x21x22, one could follow the general line of the latter table and then drop the last Column.

Single Youdens where $c=(t\pm1)/2$ - An extremely useful class of single Youdens is that where the number c of Columns is about half the number of the Treatments. This may, perhaps, be thought of as central Youdens in the sense that c is about half-way between its minimum of 2 and the critical point of t , i.e., the latin square. It takes about half the time, i.e., number of Columns that is required for a latin square or Yates rectangle. These Designs, shown in Table XIII have unrepeated Change-over and Treatments in cyclic order in the Columns. In these Designs, obviously, $t = 4m - 1$, m being the successive integers starting with unity. The 3x2x3 already shown in Table XII is, of course, omitted. For all members of the series $t \leq 31$, Designs were found except for 27x13x27 (and complementary 27x14x27) for which, by exhaustive exploration, none exist. In the case of prime numbers, $t = 4m - 1$ $(m > 0$ an integer) it is pretty plain, from the cases where $t = 43$

and $t = 47$, how things probably go if one should want to go beyond the list shown in Table XIII.

For these Youden rectangles, and for those in the immediately following section, it is impossible to control 2-space contiguity in the way possible for latin squares, t even, as previously discussed. This is impossible, because there it was necessary to rearrange Rows so that the downward differences were never repeated and that is impossible when t is necessarily odd. This impossibility is the same as that of arranging latin squares, with t odd, so that forward differences are never repeated.

Single Youdens where $f = 1$ - There has been found one other class of single Youdens that may be written in cyclic Columns with unrepeated Change-over, to wit, those where $f = 1$, i.e.,

$$c = (1 + \sqrt{4t - 3})/2 \quad .$$

They are extremely useful because the c is necessarily quite small, of magnitude $\sqrt{t}$, and so if Columns are time the work can be done very quickly. The Designs 3x2x3 and 7x3x7 belong to the present series but have been presented in earlier tables. The 3 Designs 13x4x13 etc., found, are presented in Table XIV. Together with each Design, where $t \times c \times t$ gives $f = 1$, there exists a complementary with $t \times (t - c) \times t$. It may be found readily enough but is generally not of much use because $t - c$ is too close to t . The Design, 43x7x43, would be expected in Table XIV but it cannot* be written in any way, cyclic or non-cyclic. The next highest member

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*Beall, G. A balanced incomplete block which might be expected but does not exist and other members of its respectable family. Educational Testing Service, Princeton, N. J. (in process 1971).

Table XIII- Designs where $c=(t\pm 1)/2$

7x3x7 f=1 *

Row	I	(1)	(2)	(4)
	Δ		1	2

7x4x7 f=2

Row	I	(1)	(2)	(4)	(7)
	Δ		1	2	3

11x5x11 f=2

Row	I	(1)	(2)	(4)	(7)	(11)
	Δ		1	2	3	4

11x6x11 f= 3 ✓

Row	I	(1)	(2)	(4)	(7)	(11)	(5)
	Δ		1	2	3	4	5

15x7x15 f= 3

Row	I	(1)	(2)	(5)	(3)	(9)	(6)	(11)
	Δ		1	3	13	6	12	5

15x8x15 f= 4

Row	I	(1)	(7)	(12)	(8)	(15)	(3)	(5)	(6)
	Δ		6	5	11	7	3	2	1

19x9x19 f=4

Row	I	(1)	(2)	(4)	(7)	(11)	(16)	(3)	(10)	(18)
	Δ		1	2	3	4	5	6	7	8

19x10x19 f=5

Row	I	(1)	(2)	(4)	(7)	(11)	(16)	(3)	(10)	(18)	(8)
	Δ		1	2	3	4	5	6	7	8	9

23x31x23 f=5

Row	I	(1)	(2)	(4)	(7)	(11)	(16)	(22)	(6)	(14)	(23)	(10)
	Δ		1	2	3	4	5	6	7	8	9	10

23x12x23 f=6

Row	I	(1)	(2)	(4)	(7)	(11)	(16)	(22)	(6)	(14)	(23)	(10)	(21)
	Δ		1	2	3	4	5	6	7	8	9	10	11

31x15x31 f= 7

Row	I	(1)	(2)	(4)	(7)	(11)	(16)	(22)	(29)	(6)	(15)	(25)	(5)	(17)	(30)	(13)
	Δ		1	2	3	4	5	6	7	8	9	10	11	12	13	14

31x16x31 f=8

Row	I	(1)	(2)	(4)	(7)	(11)	(16)	(22)	(29)	(6)	(15)	(25)	(5)	(17)	(30)	(13)	(28)
	Δ		1	2	3	4	5	6	7	8	9	10	11	12	13	14	15

43x21x43 f=10

Row	I	(1)	(2)	(4)	(7)	(11)	(16)	(22)	(29)	(37)	(3)	(13)	(24)	(36)	(6)	(20)	(35)	(8)	(25)	(43)	(19)	(39)
	Δ		1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20

43x22x43 f=11

Row	I	(1)	(2)	(4)	(7)	(11)	(16)	(22)	(29)	(37)	(3)	(13)	(24)	(36)	(6)	(20)	(35)	(8)	(25)	(43)	(19)	(39)	(17)
	Δ		1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21

47x23x47 f=11

Row 1	(1)	(2)	(4)	(7)	(11)	(16)	(22)	(29)	(37)	(46)	(9)	(20)	(32)	(45)	(12)	(27)	(43)	(13)	(31)	(3)	(23)	(44)	(19)
Δ		1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22

47x24x47 f=12

Row I	(1)	(2)	(4)	(7)	(11)	(16)	(22)	(29)	(37)	(46)	(9)	(20)	(32)	(45)	(12)	(27)	(43)	(13)	(31)	(3)	(23)	(44)	(19)	(42)
Δ		1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23

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*The Design, 7x3x7, will yield estimates of direct Treatment and Carry-over but provide no test of significance without repetition.

Table XIV - Single Youdens where fill, f = 1

13x4x13

Row I	(1)	(5)	(6)	(8)
Δ		4	1	2

Row I	(1)	(2)	(5)	(7)
Δ		1	3	2

21x5x21

Row I	(1)	(4)	(5)	(10)	(12)
Δ		3	1	5	2

31x6x31

Row I	(1)	(2)	(5)	(11)	(13)	(18)
Δ		1	3	6	2	5

Row I	(1)	(2)	(4)	(9)	(13)	(19)
Δ		1	2	5	4	6

Row I	(1)	(2)	(5)	(7)	(14)	(22)
Δ		1	3	2	7	8

57x8x57

Row I	(1)	(8)	(11)	(17)	(19)	(31)	(32)	(36)
Δ		7	3	6	2	12	1	4

73x9x73

Row I	(1)	(3)	(11)	(25)	(26)	(30)	(37)	(43)	(46)
Δ		2	8	14	1	4	7	6	3

91x10x91

Row I	(1)	(2)	(7)	(11)	(24)	(27)	(35)	(42)	(54)	(56)
Δ		1	5	4	13	3	8	7	12	2

133x12x133

Row I	(1)	(26)	(30)	(39)	(49)	(56)	(67)	(83)	(88)	(89)	(91)	(103)
Δ		25	4	9	10	7	11	16	5	1	2	12

of the series, 111x11x111, has not been found in extensive, but not exhaustive, exploration; there is some reason to suppose that no Design exists. The series might be profitably extended to deal with problems usually of preliminary character, where a very great many Treatments must be explored.

With regard to the writing of complementary Designs the practice may be briefly stated. Given a Design $t \times c \times t$, then complete each of the t Rows by the $t - c$ Treatments that it lacks. In this way there will be produced the balanced incomplete block pattern for $t - c$ Treatments. Rearrange these by permutations within Rows so that each Treatment occurs once and once only within each Column. The condition that a given Treatment be preceded not more than once by any other given Treatment may be added.

Intractable single Youdens - Many of the possible single Youdens shown or suggested by Table IV remain unwritten with the Treatments cyclic in the Columns, let alone written with unrepeated Change-over. They are a miscellaneous assembly where $c \neq t$, $c \neq t - 1$, $c \neq (t \pm 1)/2$ and $f \neq 1$. There are situations where a single Youden, such as 16x6x16, $f = 2$, may exist, but cannot be written in cyclic form. This particular Design can, however, be written in non-cyclic form with unrepeated Change-over, as below in Table XV.

In the series of single Youdens with $f = 2$, 2x2x2, 4x3x4, 7x4x7 and 11x5x11 belong, of course, to earlier series and for each of them a Youden Design with cyclic Columns and unrepeated Change-over has been shown. For the next member of the class, otherwise, i.e., 16x6x16, there is no cyclic arrangement; this by exhaustive exploration. For this fairly common and very useful situation, there has been found a Youden, as shown in Table XV, with

Table XV - Non-cyclic Youdens with unrepeated Change-over, i.e., 16x6x16.

a. Design

	Column					
Row	1	2	3	4	5	6
I	(8)	(9)	(1)	(13)	(6)	(7)
II	(5)	(10)	(2)	(14)	(7)	(8)
III	(6)	(11)	(3)	(15)	(8)	(5)
IV	(7)	(12)	(4)	(16)	(5)	(6)
V	(4)	(13)	(5)	(9)	(2)	(3)
VI	(1)	(14)	(6)	(10)	(3)	(4)
VII	(2)	(15)	(7)	(11)	(4)	(1)
VIII	(3)	(16)	(8)	(12)	(1)	(2)
IX	(16)	(1)	(9)	(5)	(14)	(15)
X	(13)	(2)	(10)	(6)	(15)	(16)
XI	(14)	(3)	(11)	(7)	(16)	(13)
XII	(15)	(4)	(12)	(8)	(13)	(14)
XIII	(12)	(5)	(13)	(1)	(10)	(11)
XIV	(9)	(6)	(14)	(2)	(11)	(12)
XV	(10)	(7)	(15)	(3)	(12)	(9)
XVI	(11)	(8)	(16)	(4)	(9)	(10)

b. Change-over

	Followed by															
Treatment	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
(1)		x							x	x			x	x		
(2)			x							x	x			x	x	
(3)				x							x	x			x	x
(4)	x								x			x	x			x
(5)						x			x	x			x	x		
(6)							x			x	x			x	x	
(7)								x			x	x			x	x
(8)					x				x			x	x			x
(9)	x	x			x	x				x						
(10)		x	x			x	x				x					
(11)			x	x			x	x				x				
(12)	x			x	x			x	x							
(13)	x	x			x	x								x		
(14)		x	x			x	x								x	
(15)			x	x			x	x								x
(16)	x			x	x			x					x			

non-cyclic Columns but with unrepeated Change-over. This Design is Youden in that it double-fills the comparison table. It does have unrepeated Change-over as may be seen in XVb. This is the type of Design shown by Fisher and Yates (1967), except that, of course, they have no concern with Carry-over. For the next two potential members of the series, 22x7x22 and 29x8x29, there exist no Designs, as are previously the case for 43x7x43, f = 1 , according to Hussain (1945 and 1946). Curiously enough, there is for the next member a cyclic arrangement.

37x9x37 f = 2

Row I	(1)	(2)	(4)	(8)	(18)	(25)	(30)	(36)	(26)
Δ		1	2	4	10	7	5	6	27

In the series of single Youdens with f = 3 , 3x3x3, 5x4x5, 11x6x11 and 15x7x15 belong, of course, to earlier series and for each of them a Youden Design with cyclic Columns and unrepeated Change-over has been shown. For the case of 25x9x25, there is by exhaustive examination, no such arrangement. Also probably for 31x10x31, f = 3 , there is no such arrangement. For the moment, a Design for 25x9x25, f = 3 , taken from Cochran and Cox (1957) with, however, repeated Change-over, and similarly one for 31x10x31, f = 3, from the statistical handbook of the Chemical Rubber Co., with the same shortcoming, are shown in Table XVI. Probably it is just a matter of the necessary effort to rewrite each with unrepeated Change-over.

The situation seems much the same for the series where f=4, i.e., 4x4x4, 15x8x15, 34x12x34 etc. The first 2 members belong to earlier series and have been shown. No cyclic-column Design was found for 34x12x34, f=4, although the matter was not explored thoroughly.

Table XVI - Non-cyclic Youdens with repeated Change-over when f = 3

a. 25x9x25, f = 3

Row	Column 1	2	3	4	5	6	7	8	9
I	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
II	(18)	(1)	(14)	(22)	(15)	(23)	(19)	(3)	(2)
III	(3)	(16)	(1)	(2)	(17)	(24)	(21)	(25)	(20)
IV	(4)	(7)	(10)	(1)	(21)	(20)	(11)	(14)	(15)
V	(7)	(4)	(12)	(13)	(1)	(16)	(17)	(18)	(19)
VI	(5)	(9)	(19)	(25)	(18)	(1)	(24)	(11)	(10)
VII	(9)	(5)	(13)	(20)	(23)	(21)	(1)	(12)	(22)
VIII	(6)	(8)	(16)	(11)	(22)	(17)	(10)	(1)	(23)
IX	(25)	(15)	(6)	(14)	(12)	(13)	(8)	(24)	(1)
X	(10)	(12)	(2)	(9)	(24)	(15)	(22)	(17)	(4)
XI	(11)	(13)	(4)	(23)	(25)	(9)	(14)	(2)	(16)
XII	(12)	(10)	(5)	(8)	(14)	(2)	(16)	(19)	(21)
XIII	(13)	(11)	(8)	(5)	(20)	(18)	(2)	(15)	(17)
XIV	(2)	(20)	(23)	(7)	(10)	(25)	(18)	(6)	(12)
XV	(19)	(24)	(22)	(21)	(2)	(11)	(13)	(7)	(6)
XVI	(21)	(3)	(18)	(10)	(4)	(8)	(23)	(13)	(24)
XVII	(20)	(19)	(11)	(3)	(8)	(4)	(12)	(22)	(25)
XVIII	(15)	(22)	(7)	(16)	(13)	(5)	(25)	(10)	(3)
XIX	(14)	(17)	(24)	(12)	(11)	(3)	(5)	(23)	(7)
XX	(17)	(14)	(9)	(6)	(19)	(10)	(3)	(20)	(13)
XXI	(16)	(18)	(21)	(15)	(3)	(12)	(6)	(9)	(11)
XXII	(22)	(6)	(20)	(24)	(16)	(14)	(4)	(5)	(18)
XXIII	(23)	(21)	(25)	(17)	(6)	(19)	(15)	(4)	(5)
XXIV	(8)	(25)	(17)	(18)	(7)	(22)	(9)	(21)	(14)
XXV	(24)	(23)	(15)	(19)	(9)	(7)	(20)	(16)	(8)

Note that Change-over is repeated; this matter has not been adjusted.

b. 31x10x31, f = 3

	Column									
Row	1	2	3	4	5	6	7	8	9	10
I	(1)	(2)	(4)	(8)	(9)	(11)	(15)	(16)	(18)	(28)
II	(2)	(3)	(12)	(9)	(10)	(17)	(16)	(19)	(5)	(22)
III	(3)	(4)	(20)	(10)	(17)	(13)	(6)	(18)	(11)	(23)
IV	(4)	(5)	(7)	(11)	(12)	(21)	(18)	(14)	(19)	(24)
V	(5)	(6)	(1)	(12)	(13)	(8)	(19)	(20)	(15)	(25)
VI	(6)	(7)	(13)	(16)	(14)	(9)	(20)	(21)	(2)	(26)
VII	(7)	(1)	(15)	(14)	(8)	(10)	(21)	(17)	(3)	(27)
VIII	(8)	(11)	(17)	(25)	(16)	(23)	(29)	(7)	(26)	(5)
IX	(9)	(12)	(24)	(29)	(27)	(18)	(1)	(26)	(17)	(6)
X	(10)	(13)	(18)	(19)	(29)	(25)	(2)	(27)	(28)	(7)
XI	(11)	(14)	(22)	(26)	(19)	(20)	(3)	(28)	(29)	(1)
XII	(12)	(8)	(27)	(23)	(20)	(29)	(4)	(22)	(21)	(2)
XIII	(13)	(9)	(29)	(28)	(21)	(15)	(5)	(23)	(24)	(3)
XIV	(14)	(10)	(25)	(22)	(15)	(16)	(24)	(29)	(6)	(4)
XV	(15)	(24)	(26)	(5)	(2)	(27)	(11)	(10)	(30)	(20)
XVI	(16)	(25)	(6)	(30)	(3)	(28)	(12)	(11)	(27)	(21)
XVII	(17)	(26)	(28)	(7)	(30)	(22)	(13)	(12)	(4)	(15)
XVIII	(18)	(27)	(23)	(1)	(5)	(30)	(14)	(13)	(22)	(16)
XIX	(19)	(28)	(30)	(2)	(6)	(14)	(8)	(24)	(23)	(17)
XX	(20)	(22)	(8)	(3)	(7)	(24)	(9)	(30)	(25)	(18)
XXI	(21)	(23)	(10)	(4)	(1)	(26)	(30)	(25)	(9)	(19)
XXII	(22)	(21)	(11)	(17)	(24)	(1)	(25)	(2)	(31)	(13)
XXIII	(23)	(15)	(3)	(18)	(25)	(2)	(26)	(31)	(12)	(14)
XXIV	(24)	(16)	(19)	(31)	(26)	(3)	(27)	(4)	(13)	(8)
XXV	(25)	(17)	(14)	(27)	(31)	(4)	(28)	(5)	(20)	(9)
XXVI	(26)	(18)	(5)	(21)	(28)	(31)	(22)	(6)	(8)	(10)
XXVII	(27)	(19)	(31)	(15)	(22)	(6)	(23)	(9)	(7)	(11)
XXVIII	(28)	(20)	(16)	(24)	(23)	(7)	(31)	(1)	(10)	(12)
XXIX	(29)	(30)	(2)	(6)	(4)	(5)	(7)	(3)	(1)	(31)
XXX	(30)	(31)	(9)	(13)	(11)	(12)	(10)	(8)	(14)	(29)
XXXI	(31)	(29)	(21)	(20)	(18)	(19)	(17)	(15)	(16)	(30)

Note that Change-over is repeated; this matter has not been adjusted. From, Handbook of tables for probability and statistics. Chemical Rubber Co., Cleveland, Ohio.

Paired Youdens for balance of Change-over - Table XVII shows paired Designs, 2(t x c x t); each single Youden, t x c x t , c < t , in its own right, but used together to get balanced Change-over. The single Youdens used for the combination come at least in a sense, from TableXIII. There, unrepeated Change-over was obtained but there were not enough Columns to balance the Change-over. Here, however, were c = (t + 1)/2 , t being necessarily odd, the 2(c - 1) Changes-over must exactly embrace the gamut possible. These Designs of the type 2(t x c x t) resemble somewhat the double Youdens which follow and where balance may also occur. An example of the two Youdens arises from 7x4x7. From this there may be written a Design of 2(7x4x7) which has, of course, 6 Changes-over and these may be balanced. The Design is:

Row I	(1)	(2)	(4)	(7)	Row VIII	(1)	(5)	(3)	(2)
II	(2)	(3)	(5)	(1)	IX	(2)	(6)	(4)	(3)
III	(3)	(4)	(6)	(2)	X	(3)	(7)	(5)	(4)
IV	(4)	(5)	(7)	(3)	XI	(4)	(1)	(6)	(5)
V	(5)	(6)	(1)	(4)	XII	(5)	(2)	(7)	(6)
VI	(6)	(7)	(2)	(5)	XIII	(6)	(3)	(1)	(7)
VII	(7)	(1)	(3)	(6)	XIV	(7)	(4)	(2)	(1)

Here Treatment (1) is preceded once and followed once by each other Treatment (2) through (7). All other Treatments (2) through (7) are similarly preceded and followed. If this Design is written in brief form,

			2(7x4x7)		f=4
Row I	(1)	(2)	(4)	(7)	
Δ		1	2	3	
VIII	(1)	(5)	(3)	(2)	
Δ		4	5	6	

it can be seen that since Change-over constantly changes, (1) can never be followed, or preceded, by any other Treatment twice. This design fills a paired-comparison table 4 times. Rows I through VII are a Youden rectangle with unrepeated Change-over; Rows VIII through XIV are likewise.

Table XVII - Designs for paired Youdens, 2(txcxt) with cyclic Columns and balanced Change-over

2(7x4x7) f=4

Row I	(1)	(2)	(4)	(7)
Δ		1	2	3
VIII	(1)	(5)	(3)	(2)
Δ		4	5	6

2(11x6x11) f=6

1	(1)	(2)	(4)	(7)	(11)	(5)
Δ		1	2	3	4	5
XII	(1)	(7)	(3)	(11)	(9)	(8)
Δ		6	7	8	9	10

2(15x8x15) f=4

I	(1)	(7)	(12)	(8)	(15)	(3)	(5)	(6)
Δ		6	5	11	7	3	2	1
XVI	(1)	(10)	(5)	(9)	(2)	(14)	(12)	
Δ		9	10	4	8	12	13	

2(19x10x19) f=10

Row I	(1)	(2)	(4)	(7)	(11)	(16)	(3)	(10)	(18)	(8)
Δ		1	2	3	4	5	6	7	8	9
XX	(1)	(11)	(3)	(15)	(9)	(4)	(19)	(16)	(14)	(13)
Δ		10	11	12	13	14	15	16	17	18

2(23x12x23) f=12

Row I	(1)	(2)	(4)	(7)	(11)	(16)	(22)	(6)	(14)	(23)	(10)	(21)
Δ		1	2	3	4	5	6	7	8	9	10	11
XXIV	(1)	(13)	(3)	(17)	(9)	(2)	(19)	(14)	(10)	(7)	(5)	(4)
Δ		12	13	14	15	16	17	18	19	20	21	22

2(31x16x31) f=16

Row I	(1)	(2)	(4)	(7)	(11)	(16)	(22)	(29)	(6)	(15)	(25)	(5)	(17)	(30)	(13)	(28)
Δ		1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
XXXII	(1)	(17)	(3)	(21)	(9)	(29)	(19)	(10)	(2)	(26)	(20)	(15)	(11)	(8)	(6)	(5)
Δ		16	17	18	19	20	21	22	23	24	25	26	27	28	29	30

The foregoing judicious combination of two single Youdens raises temptations that should be firmly resisted. By the combination of various numbers of single Youdens much could be achieved. In connection with any given Youden where the Change-over is unbalanced but unrepeated it is, naturally, possible to combine several in an appropriate way to achieve balance. The only problem is that of having enough experimental units. Consider thus the simple Yates, 4x3x4, in 3 forms, as follows:

Row I	(1)	(2)	(4)
Δ		1	2

Row V	(1)	(3)	(4)
Δ		2	1

Row IX	(1)	(4)	(3)
Δ		3	3

	Treatment			
After	(1)	(2)	(3)	(4)
(1)		x	x	
(2)			x	x
(3)	x			x
(4)	x	x		

	Treatment			
After	(1)	(2)	(3)	(4)
(1)		x	x	
(2)			x	x
(3)	x			x
(4)	x	x		

	Treatment			
After	(1)	(2)	(3)	(4)
(1)				xx
(2)	xx			
(3)		xx		
(4)			xx	

or in total

	Treatment			
After	(1)	(2)	(3)	(4)
(1)		xx	xx	xx
(2)	xx		xx	xx
(3)	xx	xx		xx
(4)	xx	xx	xx	

This is just an application of an idea developed at some length by Patterson and Lucas (1962) where several variants of a Design may be combined to give handsomely balanced Change-over. Such temptation should, however, be resisted because there is suggested a complexity that is in fact quite trivial but which may disconcert and alarm men who might otherwise use new techniques.

Double Youdens where $c = (t \pm 1)/2$ - It was proposed in Chap. II to write Designs called double Youdens, of type $t \times c \times 2t$, or in general cases where $r = 2t$ as in the discussion about Equ. (3). We cannot write, for instance, a 9x4x9 Youden rectangle because it yields, in the sense of the comparison table of Table IIb, or elsewhere, $18C_2^4 = 108$ comparisons within Rows to fill the $2C_2^9 = 72$ positions in the comparison table, i.e., there would be 3/2 fill. Accordingly, we write 18 Rows or 9x4x18 to get 216 comparisons to fill 72 positions thrice; $f = 3$. For the moment attention is restricted to what were previously, in connection with single Youdens, as in Table XIII, called central cases, i.e., where $c = (t \pm 1)/2$. Such Designs are shown in Table XVIII. The particular Design of 9x4x18 is illustrated with data later in Table XXXI and it is again discussed, in another connection, in Table XXXV. In this table all the Designs have cyclic Columns, or perhaps one should say double-cyclic. Change-over is, of course, always unrepeated but in some cases, such as 3x2x6, it happens also to work out balanced. These are cases where $2(c - 1)$, the number of Changes-over occurring equals $t - 1$, the highest number possible with repetition. These central double Youdens of both kinds are, disappointingly, practically the only double Youdens with cyclic Columns that exist in the realm of $t \leq 36$.

There are, among these central double Youdens, two kinds, as follows: First, those where $c = (t - 1)/2$. Then the number of Changes-over must be less than the number of Treatments with which we should like to compare, say, Treatment (1). Accordingly, the Change-over will not be balanced although we may suppose that it should be unrepeated, which will commonly be possible. An example is Design 9x4x18, $f = 3$, of Table XVIII, where

Row I	(1)	(2)	(4)	(8)
II	(2)	(3)	(5)	(9)
III	(3)	(4)	(6)	(1)
IV	(4)	(5)	(7)	(2)
V	(5)	(6)	(8)	(3)
VI	(6)	(7)	(9)	(4)
VII	(7)	(8)	(1)	(5)
VIII	(8)	(9)	(2)	(6)
IX	(9)	(1)	(3)	(7)

Row X	(1)	(6)	(3)	(2)
XI	(2)	(7)	(4)	(3)
XII	(3)	(8)	(5)	(4)
XIII	(4)	(9)	(6)	(5)
XIV	(5)	(1)	(7)	(6)
XV	(6)	(2)	(8)	(7)
XVI	(7)	(3)	(9)	(8)
XVII	(8)	(4)	(1)	(9)
XVIII	(9)	(5)	(2)	(1)

Treatment (1) is followed only by (2), (3), (5), (6), (7) and (9). Each other Treatment is necessarily followed by only some of the other Treatments. Within a given half of the Design, the comparisons within Rows involve two Treatments not less than once and not more than twice, but the matter is not redressed in the other half. Secondly, there are those central double Youdens where the number of Changes-over must be exactly the number of Treatments with which we should like to compare, say, Treatment (1). Accordingly, the Change-over will be balanced. An example is 9x5x18, f = 5 , of Table XVIII, where:

Row I	(1)	(2)	(4)	(7)	(3)
II	(2)	(3)	(5)	(8)	(4)
III	(3)	(4)	(6)	(9)	(5)
IV	(4)	(3)	(7)	(1)	(6)
V	(5)	(6)	(8)	(2)	(7)
VI	(6)	(7)	(9)	(3)	(8)
VII	(7)	(8)	(1)	(4)	(9)
VIII	(8)	(9)	(2)	(5)	(1)
IX	(9)	(1)	(3)	(6)	(2)

Row X	(1)	(9)	(6)	(4)	(8)
XI	(2)	(1)	(7)	(5)	(9)
XII	(3)	(2)	(8)	(6)	(1)
XIII	(4)	(3)	(9)	(7)	(2)
XIV	(5)	(4)	(1)	(8)	(3)
XV	(6)	(5)	(2)	(9)	(4)
XVI	(7)	(6)	(3)	(1)	(5)
XVII	(8)	(7)	(4)	(2)	(6)
XVIII	(9)	(8)	(5)	(3)	(7)

Treatment (1) is followed once by each of (2) through (9). Each other Treatment is similarly followed by all alternative Treatments. The comparisons within Rows involved any two Treatments not less than twice and not more than thrice in one half. In both halves, each comparison occurs five times altogether.

Table XVIII - Designs for double Youdens, txcx2t, with cyclic Columns and unrepeated (balanced) Change-over where c=(t±1)/2

3x2x6 f=2

Row I	(1)	(2)
Δ		1
IV	(1)	(3)
Δ		2

balanced

5x2x10 f=1

Row I	(1)	(2)
Δ		1
VI	(1)	(4)
		3

unrepeated

5x3x10 *

none

9x4x18 f=3

Row I	(1)	(2)	(4)	(8)
Δ		1	2	4
X	(1)	(6)	(3)	(2)
Δ		5	6	8

unrepeated

9x5x18 f=5

Row I	(1)	(2)	(4)	(7)	(3)
Δ		1	2	3	5
X	(1)	(9)	(6)	(4)	(8)
Δ		8	6	7	4

balanced

13x6x26 f=5

Row I	(1)	(2)	(4)	(7)	(11)	(3)
Δ		1	2	3	4	5
XIV	(1)	(9)	(6)	(13)	(11)	(7)
Δ		8	10	7	11	9

unrepeated

13x7x26 f=7

Row I	(1)	(2)	(4)	(11)	(3)	(9)	(7)
Δ		1	2	7	5	6	11
XIV	(1)	(9)	(6)	(2)	(5)	(4)	(8)
Δ		8	10	9	3	12	4

balanced

17x8x34 f=7

Row I	(1)	(2)	(4)	(7)	(11)	(16)	(5)	(12)
Δ		1	2	3	4	5	6	7
XVIII	(1)	(9)	(6)	(5)	(15)	(7)	(2)	(17)
Δ		8	14	16	10	9	12	15

unrepeated

17x9x34 f=9

Row I	(1)	(5)	(7)	(8)	(11)	(9)	(17)	(13)	(10)
Δ		4	2	1	3	15	8	13	14
XVIII	(1)	(13)	(3)	(12)	(6)	(11)	(17)	(16)	(9)
Δ		12	7	9	11	5	6	16	10

balanced

21x10x42 f=9

Row I	(1)	(2)	(4)	(3)	(6)	(12)	(10)	(17)	(9)	(13)
Δ		1	2	20	3	6	19	7	13	4
XXII	(1)	(11)	(6)	(2)	(14)	(8)	(17)	(10)	(15)	(12)
Δ		10	16	17	12	15	9	14	5	18

unrepeated

21x11x42 f=11

Row I	(1)	(2)	(4)	(3)	(6)	(11)	(7)	(14)	(8)	(17)	(15)
Δ		1	2	20	3	5	17	7	15	9	19
XXII	(1)	(12)	(18)	(5)	(2)	(14)	(3)	(17)	(21)	(16)	(8)
Δ		11	6	8	18	12	10	14	4	16	13

balanced

29x14x58 f=13

Row I	(1)	(29)	(6)	(8)	(9)	(12)	(10)	(15)	(19)	(13)	(24)	(17)	(25)	(16)
Δ		28	6	2	1	3	27	5	4	23	11	22	8	20
XXX	(1)	(8)	(5)	(18)	(7)	(23)	(19)	(9)	(21)	(13)	(27)	(15)	(10)	(20)
Δ		7	26	13	18	16	25	19	12	21	14	17	24	10

unrepeated

29x15x58 f=15

Not completed

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*It is possible to write the Design:

	5x3x10		f=3
Row I	(1)	(2)	(4)
Δ		1	2
VI	(1)	(5)	(2)
Δ		4	2

but the Change-over is unavoidedly repeated. A Treatment, say (1), is preceded by (4) twice and (3) not at all. So if there is any Carry-over it is confounded more than one would like with direct Treatment effect. Accordingly, if something of this sort must be used, one or the other halves of this Design may be employed and the lameness made up in analysis.

It was earlier remarked that apparently the writing of cyclic single Youdens with unrepeated Change-over would be extremely simple for t of almost any size, when t = 4m - 1, m an integer, and is prime. Unfortunately, the writing of comparable double Youdens, when t = 4m + 1 and is prime, is not so simple; to write the second half is hard.

Double Youdens, non-central -- There seem to exist very few double-cyclic Youdens, with unrepeated Change-over, other than those of Table XVIII. It might have been anticipated that there would be, further, a series of double Youdens, of desired character, with f = 1 , in analogy with single Youdens. In fact, there are but two--5x2x10 of Table XVIII and

	13x3x26			f=1
Row	I	(1)	(3)	(9)
	Δ		2	6
	XIV	(1)	(5)	(4)
	Δ		4	12

unrepeated

but 25x4x50, f=1, cannot be written in 4 Columns of 25 Treatments, twice over--there is no solution--this by exhaustive examination. This seems a little curious when we consider the series for single Youdens where f = 1 . The next member of the series, 41x5x82, f = 1 was not investigated. There are, of course, no double Youdens with f = 2 ; f is necessarily odd. Double Youdens where f = 3 can be conceived as belonging to the series, 5x3x10, 9x4x18, 21x6x42, 29x7x58, etc., f = 3 . The first 2 members belong to earlier series and for each of them a double Youden Design with cyclic Columns and unrepeated Change-over exists and has already been shown. Exhaustive exploration of the double Youden 21x6x42 showed there is no Design with cyclic Columns. The Design for 29x7x58 was not explored exhaustively.

For the intractable double Youdens, with c small as in 25x4x50 and 21x6x42, there should be found, if that is the best that can be done, solutions where the Treatments are not cyclic in the Columns but the Change-over is unrepeated. Something like the Design of Table XVa for 16x6x16 is required.

Whenever it is possible to write a Youden square with cyclic Columns as t x c x t then it is possible to write a conjugate Design t x (t - c) x t . Thus the Design 13x10x26, f = 15 , conjugate to 13x3x26 can be written easily enough. While, however, the Design for c < (t - 1)/2 is useful that for t - c is unattractive. The number of Changes-over must exceed the number of Treatments, but not be an integer multiplier of that number. Thus 13x10x26 must have 18 Changes-over when there are only 12 different, so that 6 Changes-over must be repeats. For such a situation we shall not attempt to find a Design. It would not be worthwhile or of practical value.

Single Change-over - The general lines on which single Change-over Designs, where Treatments are paired within an experimental unit, are written were indicated at the time of their introduction in Chap. I. They will not be listed in particular like the higher Youden Designs. To recapitulate, for t odd one may write along the lines of the model for t = 5 , as

	5x2x10		f = 1
Row I	(1)	(2)	
Δ		1	
VI	(1)	(4)	
Δ		3	

Here every comparison occurs once, f = 1, and each Treatment (1) through (5) occurs equally in each Column. Automatically, the Change-over is unrepeated although necessarily unbalanced. If balanced Change-over is desired for t odd, and there are enough Rows possible, such may be achieved by some Design along the lines appropriate for t = 5, i.e.,

	5x2x20	f = 2
Row I	(1)	(2)
	Δ	1
VI	(1)	(3)
	Δ	2
XI	(1)	(4)
	Δ	3
XVI	(1)	(5)
	Δ	4

In order to get a given Treatment the same number of times in each Column for t even it is necessary to write a larger Design which is most conveniently in the form, as for t = 4:

	4x2x12	f=2
Row I	(1)	(2)
	Δ	1
V	(1)	(3)
	Δ	2
IX	(1)	(4)
	Δ	3

which has, automatically and always, balanced Change-over.

V. Analysis of data from latin squares

Page

Special features of the latin square 100

Analysis for direct Treatment effects in a single latin square . . . 101

Analysis for direct affects in repeated Design 109

Breaking treatment variability up 122

Analysis with allowance for Carry-over in latin squares with conditioning Period 128

Analysis with allowance for Carry-over in latin squares without conditioning Period 146

Explicit solution for simple cases of Carry-over 156

Latin squares with missing Rows 159

Latin squares with missing cells 171

Special features of the latin square - Latin squares are design-wise a very special case of Youdens. Not only for this reason are they worthy of some special attention but they are, of course, widely known and commonly employed. They are a very desirable design for change-over experiments provided that t the number of Columns, or commonly the amount of time required, is practical. The latin squares are easily understood by men innocent of Statistics and are easily analyzed. As, however, t becomes great, Youden rectangles, $c < t$, have to be employed.

The peculiarity of latin squares is that the experiment is so organized that one's estimate of any effect, be it in Row, Column or Treatment, contains, in a nice balanced way, all other possible effects. Thus a given Row contains an item in each Column once, and once only, and each Treatment once, and once only. Similarly, a given Treatment is represented in each Row once, and once only, and in each Column once, and once only. Similarly, is the effect of a Column disposed. Rows, Columns and Treatments are said to be orthogonal. This orthogonality accounts for many of the steps that are taken in handling latin squares and in the form in which their analysis is presented. Unfortunately, people have tried to extend those steps and that form to cases when the effects are not orthogonal and the arithmetic contortions are extreme.

The data from a single latin square, with or without consideration for Carry-over, may be analyzed by the procedure appropriate for Youdens in general. These methods are developed and illustrated at length in the subsequent discussion and a program for electronic computer is shown in the Appendix. Alternatively, it may be analyzed by special methods, i.e., analysis of variance, which are widely shown in the literature and are perhaps more simple than those appropriate to Youdens, in general. Let us say that they are the more simple so long as Carry-over is not involved.

To some extent the procedure chosen will depend on the background of the biometrician and, to some extent, on the equipment available to him. He may use the general Youden program, if this is convenient as in a program on an electronic computer. Why bother with a special program for the latin square? If the business must be done on a desk calculator, he is more likely to use the special methods, which were, after all, designed for a desk calculator or even for pencil work in 1925.

Analysis for direct Treatment effects in single latin square - The traditional analysis of data, for direct treatment effects, from a latin square is very well known. Let us then record it briefly and illustrate it with the data of Table VI, a 6x6x6. It happens to have balanced Carry-over, as was taken up when the Design was introduced, and to be arranged with constant forward differences as in Table IX. As was discussed in connection with that, this is effectively the same thing as Treatments cyclic in Columns, none of these things is, however, of moment in the analysis for direct effects.

In the traditional approach that we are discussing it is usual to conceive the 36 observations to be something like

$$y_{ijk} = \mu + \alpha_i + \beta_j + \gamma_k + \varepsilon_{ijk} \qquad (7)$$

where μ is a general constant, α_i is the effect of the i^{th} $(i = 1 \ldots t)$ Row, β_j of the j^{th} $(j = 1 \ldots t)$ Column, k^{th} $(k = 1 \ldots t)$ Treatment and ε_{ijk} the effect of extraneous factors. Thus in the 9th cell of Table XIX

$$\begin{aligned} y_{236} &= 64.2 \\ &= \mu + \alpha_2 + \beta_3 + \gamma_6 + \varepsilon_9 \end{aligned} \qquad (8)$$

where α_2 is the effect of the second Row, where the observation occurred, β_3 is the effect of the third Column, γ_6 the effect of the sixth Treatment. It is usual to assume that ε_{ijk} is normal, distributed with standard deviation σ about zero. (The meaning of this matters little.) It is assumed that the values of ε_{ijk} are uncorrelated. It is further supposed that

$$\sum_{i=1}^{t} \alpha_i = \sum_{j=1}^{t} \beta_j = \sum_{k=1}^{t} \gamma_k = 0 \cdot \qquad (9).$$

This last equation is of the greatest importance for it is constantly appealed to in subsequent discussion. It must be noted that the whole structure of traditional analysis of latin squares is based upon the additive assumption of Equ. (7). Whether they are additive is not usually investigated. We can only say that the model seems to have worked out well in an historic way.

On the basis of Equ. (7) and (9), the estimates usual in a latin square may be written very simply in terms of totals for a given Row, Column, Treatment or overall, respectively of

$$\left.\begin{aligned} T_i &= \sum_{t} y_{ijk} && (\ i \ \text{ common}) \\ T_j &= \sum_{t} y_{ijk} && (\ j \ \text{ common}) \\ T_k &= \sum_{t} y_{ijk} && (\ k \ \text{ common}) \\ G &= \sum_{t^2} y_{ijk} && \end{aligned}\right\} \qquad (10)$$

The estimate of the general level of response is

$$\hat{\mu} = G/t^2 \quad . \tag{11}$$

The estimate of the effect of the k^{th} Treatment is

$$\hat{\gamma}_k \quad \text{or} \quad (\hat{k}) = (T_k/t) - G/t^2 \; , \tag{12}$$

i.e., the estimate of the effect of the k^{th} Treatment is the average for all observations under that Treatment less the average G/t^2 , for all observations. This is what is known as a least squares estimate although let us not bother here what that means or why we should want it. Let us simply comfort ourselves in the fact that most statistical estimates are best made by setting up least squares estimates. Even when more general methods of estimating are used, they usually include least squares method for some special case. The actual estimates for Treatments are shown in Table XIXb. They are labelled contributions; they are often thought of as the amount each mean differs from $\hat{\mu}$. The means or averages are quantities often more useful for practical reporting. Equations similar to Equ. (12) can be written for Rows and Columns if they should be needed.

A matter of more difficulty, although of less consequence than is often supposed, is that of calculating the significance of the variability among treatment averages. The question is whether they vary as much as they do just by accident, as judged from the general instability of the experimental results. Traditionally, there is found the reduction in variability, or as it is called, the sums of squares, due to Rows, Columns and Treatments respectively from

$$\left.\begin{aligned} S_R &= (t \sum_{i=1}^{t} T_i^2 - G^2)/t^2 \\ S_C &= (t \sum_{j=1}^{t} T_j^2 - G^2)/t^2 \\ S_T &= (t \sum_{k=1}^{k} T_k^2 - G^2)/t^2 \end{aligned}\right\} . \qquad (13)$$

What is called the total sum of squares is found as

$$S_G = (t^2 \sum_{t^2} y_{ijk}^2 - G^2)/t^2 \quad . \qquad (14)$$

Finally there is found the residual variability or that due to extraneous factors (which is often called the sum of squares for error)i.e.,

$$S_E = S_G - S_R - S_C - S_T \quad . \qquad (15)$$

Each sum of squares has associated with it what is called the degrees of freedom. These are one less than the number of items involved. Thus, since there are 6 Treatments there are 5 degrees of freedom. The reduction by unity is associated with the conditions of Equ. (9). The analysis of sums of squares together with the analysis of mean squares, or the analysis of variance, is as shown in Table XIX. Values of F, for testing significance, are found by dividing error mean square into mean square for each of the superior lines. These values are referred to Table L. This is, of course, a very well-known test, it is to the point that the estimate of random standard error from among say Treatments is greater than from error to a degree that could hardly happen by accident. Hence we argue that the estimates of Treatments vary more than could reasonably be expected by chance. Just where one draws the line is to some extent a

Table XIX- Analysis of latin square for direct treatment effects only

a. Data collected, repeated from Table VI

		Week							
Group	0	1	2	3	4	5	6	Sum	Mean
I	((1))	(1)46.4	(3)45.8	(2)40.8	(5)62.4	(6)59.9	(4)61.7	317.0	52.8
II	((2))	(2)60.9	(4)59.2	(3)44.9	(6)64.2	(1)55.3	(5)52.0	336.5	56.1
III	((3))	(3)50.0	(5)50.0	(4)64.2	(1)60.9	(2)58.4	(6)53.3	336.8	56.1
IV	((4))	(4)63.7	(6)72.0	(5)71.7	(2)57.3	(3)53.3	(1)52.7	370.7	61.8
V	((5))	(5)48.8	(1)50.4	(6)56.4	(3)58.9	(4)65.6	(2)64.2	344.3	57.4
VI	((6))	(6)63.2	(2)58.7	(1)51.8	(4)49.6	(5)49.6	(3)53.6	326.5	54.4
Sum		333.0	336.1	329.8	353.3	342.1	337.5	2031.8	
Mean		55.5	56.0	55.0	58.9	57.0	56.2		56.4

b. Treatment estimates

	$\hat{(1)}$	$\hat{(2)}$	$\hat{(3)}$	$\hat{(4)}$	$\hat{(5)}$	$\hat{(6)}$
Contrib.	-3.52	+.28	-5.36	+4.23	-.69	+5.06
Mean	52.92	56.72	51.08	60.67	55.75	61.50
Sum	317.5	340.3	306.5	364.0	334.5	369.0

c. Traditional analysis of variance

Source	d.f.	Sum Squares	Mean Square
Rows	t-1	S_R	$S_R/(t-1)$
Columns	t-1	S_C	$S_C/(t-1)$
Treatments	t-1	S_T	$S_T/(t-1)$
Residual	(t-1)(t-2)	S_E	$S_E/(t-1)(t-2)$
Total	t^2-1	S_G	

Source	d.f.	Sum Squares	Mean Square	F
Rows or Groups	5	280.5856	56.1171	1.04 N.S.
Columns or Weeks	5	57.4322	11.4864	.21 N.S.
Treatments	5	510.7722	102.1544	1.90 N.S.
Residual	20	1074.9356	53.7468	
"Total"	35	1923.7256		

d. General and reasonable analysis for significance

Factors	d.f.	Residual Variability (Squares)
$\hat{\mu}$, Rows & Columns (control)	25	1585.7078
Control factors plus Treatment	20	1074.9356

$$F_{5,20} = \frac{(1585.7078 - 1074.9356)/(25-20)}{1074.9356/20} = 1.90 \text{ N.S.}$$

matter of disposition. Usually it is drawn at 5%; we say there is only one chance in 20 that the variability among Treatments could be so great. As is, however, shown in Table L, one may draw the line at 1% or .1%.

It may be noted that Table XIXd has been added for the sake of comparison with subsequent work, although its supporting discussion is necessarily deferred. The quantity 1074.9356 is easily recognizable in Table XIXc and the quantity 1585.7078 = 1074.9356 + 510.7722.

The ideal of classical experimental design is that the several categories of a situation are so disposed that their effects are entirely unconfused or orthogonal and are readily estimated free of each other. The categories may be Men, Periods and Treatments. In the earliest Designs to appear, historically, i.e., the latin square, all Men are present in each Period and try all Treatments. Then one can form a tidy 3-dimension table. In saying that the systematic elements, Rows, Columns and Treatments are orthogonal, we mean that if one were systematically to add something to any one of them the relative standing of the others would be unaffected. Thus, if in Table VI, some ill-disposed person were to add 12 to all observations in Row or Group I, but there only, the data would become:

	Week					
Groups	1	2	3	4	5	6
I	(1) 58.4	(3) 57.8	(2) 52.8	(5) 74.4	(6) 71.9	(4) 73.7
II	(2) 60.9	(4) 59.2	(3) 44.9	(6) 64.2	(1) 55.3	(5) 52.0
III	(3) 50.0	(5) 50.0	(4) 64.2	(1) 60.9	(2) 58.4	(6) 53.3
		etc.		etc.		

The mean for Group I would, of course, be increased by 12 and the contributions for Groups would be generally disturbed. Then the treatment means and contributions would be

	$(\hat{1})$	$(\hat{2})$	$(\hat{3})$	$(\hat{4})$	$(\hat{5})$	$(\hat{6})$
Mean	54.92	58.72	53.08	62.67	57.75	63.50
Contrib.	-3.52	+.28	-5.36	+4.23	-.69	+5.06

The means have been increased by 2 over what they should have been from Table VI but it can be seen that the treatment contributions are quite unaffected from their values in Table XIX. Treatment is orthogonal to Group.

The quality of orthogonality is very important to the latin square. It is for this reason that the effects of the categories are found simply as the marginal averages. The problem of determining significance, as illustrated above, is correspondingly simple. Unfortunately, in the real world to be discussed in following chapters, the effects are rarely orthogonal. So both the business of estimating effects and testing significance has to be looked at in a more serious way. This is done below.

The procedure of Table XIXc as traditionally carried out to make an analysis of variance was in 1925 singularly fresh and powerful but in the year of this writing (the 44th year of statistical grace) it seems more than a little stilted and awkward. In the first place it makes no distinction between the control factors, Rows and Columns and the experimental factor, Treatments, that is to be tested. Usually we do not want to know the effects, in themselves, of Groups or Periods but simply want to get them out of the picture. They may be regarded as factors necessary for the experimental control of variability. A novice could hardly realize this because these estimates or the mean squares that correspond to them are handled in exactly the same way as those for Treatments which are of prime interest. In a cautionary way, it may be added that circumstances do control cases and if anyone should want to know the significance of Rows or Columns it can be handled in exactly the same way as that for Treatments or Carries-over. The procedure is illustrated for one problem, that of Table XXXIII which deals with the full and what seems best method of analysis for Youden rectangles. In the second place the concept that has been called residual variability is commonly called "error." The concept of "error" apparently comes from the physical sciences where all relationships are supposed to be in functions (particularly if one leave out the aberrant observations). What passes for error is perhaps best thought of as the effect of extraneous factors. It is variability in the results, y , that cannot be accounted for by the classification of data into Rows, Columns, Treatments and possibly Carry-over.

There is a curious effect that is encountered fairly regularly in change-over experiments when they are subject to considerable Carry-over and that is that the Columns or Periods seem too uniform. This can be seen in Table XIXc which is full analysis of variance. The effect of Columns or Weeks is taken out as if it were an experimental factor and required testing for significance. It will be seen that its "mean square" is only 11.49 in contrast to residual mean square of 53.75. Such happens pretty regularly. The reason seems to be that since each Treatment occurs once and once only in the preceding Column, therefore, each Carry-over occurs once and once only in the given Column. The same is true in all Columns and accordingly the Carry-over contributes not at all to the variation among Columns. It may, however, be contributing very heavily to the residual variability, which may thus be the greater when no allowance is made for Carry-over.

Analysis for direct affects in repeated Design - In practice one often sets up multiple latin squares (t even), i.e., repeated in the sense of the present discussion and by this we mean not that a given Treatment is repeated, or as is generally said replicated, but the whole square may be repeated. Thus one may set up a Design, from Table IX, on 4 Groups of men as follows:

	Period			
Group	1	2	3	4
I	(1)	(2)	(3)	(4)
II	(2)	(4)	(1)	(3)
III	(3)	(1)	(4)	(2)
IV	(4)	(3)	(2)	(1)

and then repeat this 5 times so that 20 Groups of men are covered, as has been done in Table XX. It is, of course, realized that it would be somewhat more sophisticated to give each fresh set of 4 Groups a fresh Design, so far as possible. It would even probably yield better results. The stumbling block is one of practicality. It is better not to write out elaborate instructions for other people to execute and that will require proofing by the writer. Also in actual practice the result would probably be incomplete in some way, with cells missing or even lines missing, so that the total Design would then become quite intricate and its analysis involved. In Table XX which was chosen for illustrative purposes, the Design is complete. It will be noted that Table XXa is concluded by quantities with the label "Sum sq." These are simply the sum of the squares of the observations 53, 57, 50 etc. in, say, Column 1 and are added to save work later for anyone choosing to verify the calculations. Such quantities were shown at the foot of Table XIXa.

The analysis for direct treatment effects in paired latin squares, 2(t x t x t), as in Table X, which occur when t is odd, must, of course, be just the same as in a once repeated latin square Design.

It is possible to make for this situation of multiple latin squares an analysis of variance very similar to that for single latin squares, as shown in Table XXd. Such is, indeed, the general procedure in the literature. This analysis has no longer, however, the justification of convenience. Its generalization leads to increasing complications to the point where, for the more intractable cases that arise in practice, analysis becomes impossible. Accordingly, it seems best to turn in the direction of methods suitable for this and all later problems. It is assumed that a fundamental structure of general row, column, treatment and extraneous effects exists as in Equ. (7) and that row, column and treatment effects add to zero, as in Equ. (9). Now a man, reasonable, but innocent of Biometry, let alone the Calculus, required to form some estimate of the quantity μ in Equ. (7) from the data of Table XX would presumably proceed along a line of thought that may be conjured up. He would argue that in view of Equ. (7) every one of the 80 observations in the table contains the quantity μ so that none may be safely ignored without throwing away a potential bit of information on μ. Surely the sum or the average of the 80 observations would give him some idea of μ? Now starting out to consider what we get besides μ in the sum he would find

$$\left.\begin{aligned}
53 &= \mu + \alpha_1 + \beta_1 + \gamma_1 + \varepsilon_1 \\
58 &= \mu + \alpha_1 + \beta_2 + \gamma_2 + \varepsilon_2 \\
58 &= \mu + \alpha_1 + \beta_3 + \gamma_3 + \varepsilon_3 \\
53 &= \mu + \alpha_1 + \beta_4 + \gamma_4 + \varepsilon_4 \\
57 &= \mu + \alpha_2 + \beta_1 + \gamma_1 + \varepsilon_5 \\
55 &= \mu + \alpha_2 + \beta_2 + \gamma_2 + \varepsilon_6 \\
&\cdots\cdots\cdots\cdots\cdots\cdots \\
58 &= \mu + \alpha_{20} + \beta_4 + \gamma_1 + \varepsilon_{80}
\end{aligned}\right\} \qquad (16)$$

whence

$$80\mu + 4(\alpha_1 + \alpha_2 + \alpha_3 +\ldots+ \alpha_{20}) + 20(\beta_1 + \beta_2 + \beta_3 + \beta_4) + 20(\gamma_1 + \gamma_2 + \gamma_3 + \gamma_4)$$
$$+ \varepsilon_1 + \varepsilon_2 + \varepsilon_3 +\ldots+ \varepsilon_{80}$$
$$= 4241 \tag{17}$$

Upon reflection, remembering Equ. (9), he would simplify and say

$$80\mu + \varepsilon_1 + \varepsilon_2 + \varepsilon_3 + \ldots + \varepsilon_{80} = 4241 \quad . \tag{18}$$

Now since, as can be shown, there is no sure way of estimating ε_1 , ε_2 , ε_3 etc. he could only hope they would to a considerable extent cancel, some being positive and some negative, and otherwise forget them. So finally he would come to the comfortable,

$$80\hat{\mu} = 4241 \tag{19}$$

which, of course, results in the average

$$\hat{\mu} = 53.01 \tag{20}$$

(Note the little hat means that even when all is done he would not know μ , but only have an estimate subject to accidental variations.) Next suppose the reasonable man wanted to get an estimate of the control factor of Group. He would first estimate the level of satisfaction for Group I. He would argue that in view of Equ. (7) every one of the first 6 observations in the table contains the quantity α_1 so that none may be safely ignored without throwing away a potential bit of information on α_1 . Surely the sum or the average of the first 6 observations would give him some idea of α_1 ? Now, starting out to consider what he would get besides α_1 in the sum, from the first four of Equ. (16),

$$222 = 4\mu + 4\alpha_1 + (\beta_1 + \beta_2 + \beta_3 + \beta_4) + (\gamma_1 + \gamma_2 + \gamma_3 + \gamma_4) + \varepsilon_1 + \varepsilon_2 + \varepsilon_3 + \varepsilon_4 \tag{21}$$

or upon reflection, remembering Equ. (9) he would say

$$4\alpha_1 + 4\mu + \varepsilon_1 + \varepsilon_2 + \varepsilon_3 + \varepsilon_4 = 222 . \tag{22}$$

Now since, again, the only thing he can do with ε_1 , ε_2 etc. is drop them and write comfortably

$$4\hat{\alpha}_1 + 4\hat{\mu} = 222 . \tag{23}$$

Let him then write for Group I through XX, the equations:

$$\left.\begin{array}{r} 4\hat{\alpha}_1 + 4\hat{\mu} = 222 \\ 4\hat{\alpha}_2 + 4\hat{\mu} = 211 \\ 4\hat{\alpha}_3 + 4\hat{\mu} = 226 \\ 4\hat{\alpha}_4 + 4\hat{\mu} = 225 \\ \cdots\cdots\cdots\cdots \\ 4\hat{\alpha}_{20} + 4\hat{\mu} = 220 \end{array}\right\} . \tag{24}$$

In more or less the same way he would get equations for the control factor of Period as:

$$\left.\begin{array}{r} 20\hat{\beta}_1 + 20\hat{\mu} = 1121 \\ 20\hat{\beta}_2 + 20\hat{\mu} = 1048 \\ 20\hat{\beta}_3 + 20\hat{\mu} = 1029 \\ 20\hat{\beta}_4 + 20\hat{\mu} = 1043 \end{array}\right\} . \tag{25}$$

In this way he would get estimates:

$$\left.\begin{array}{l} \begin{array}{cccccc} \hat{\alpha}_1 & \hat{\alpha}_2 & \hat{\alpha}_3 & \hat{\alpha}_4 & \cdots & \hat{\alpha}_{20} \\ \hline +2.49 & -.26 & +3.49 & +3.24 & \cdots & +1.99 \end{array} \\ \\ \begin{array}{cccc} \hat{\beta}_1 & \hat{\beta}_2 & \hat{\beta}_3 & \hat{\beta}_4 \\ \hline +3.04 & -.61 & -1.56 & -.86 \end{array} \end{array}\right\} . \tag{26}$$

By a similar argument, he would get equations for the experimental Treatments as follows:

$$\left.\begin{aligned} 20\hat{\gamma}_1 + 20\hat{\mu} &= 1040 \\ 20\hat{\gamma}_2 + 20\hat{\mu} &= 1075 \\ 20\hat{\gamma}_3 + 20\hat{\mu} &= 1092 \\ 20\hat{\gamma}_4 + 20\hat{\mu} &= 1034 \end{aligned}\right\} \qquad (27)$$

yielding estimates of Treatment effects, as in Table XX b. All the treatment estimates, or contributions, $\hat{\gamma}_k$, are shown in Table XX b as $(\hat{k})$, which is a more convenient way of reporting them. As previously in Table XIXb, there are added the means for the Treatments.

It must seem that in the preceding passage a mountain has labored mightily to bring forth a very small mouse. The point of view is important because it applies to later cases of more involved design or imperfectly executed design. One can set up quite easily oneself what are called "least squares" estimates for all the effects in a latin square without getting all involved in the elaborate business of partial differentiation usually given in their derivation. These equations are, at least for all problems in the present book, very easily set up along lines that a reasonable man would follow anyhow. For a given Treatment he would form the total or average result, although we do adjust it a little for any effect of Rows, Columns, etc., which happened to become involved from the nature of the Design. This approach, it should be understood clearly, leads to results identical with those usual. The approach is convenient and meaningful in an experimental way.

While estimates of treatment effects should probably be considered the main output of an experiment, such as that of Table XX, i.e., the results of Table XXb, it is often important to test the significance of those estimates. Important or no it is generally done as in Table XXc. It is often necessary to go beyond these and the estimates of effects as in Equ. (26), to the question of the chance variation in these estimates and the general chance variation in the observations that is not accounted for by the systematic effects. Here the intuition of the reasonable man would be inadequate and it would be necessary for him to appeal to a biometrician. And this is a good thing because it makes it possible for biometricians to earn a living. With regard to the residual variability it is necessary to think of the problem in two steps. First, what would be the residual variability if one made allowance only for the control factors, Rows and Columns, and secondly if one made allowance for the factor of experimental Treatment. The vital question is whether it is much reduced by the inclusion of Treatments because then we shall be inclined to regard them as significant.

Consider the first problem of getting the variability, residual on the control factors. Here we may take advantage of a rule without attempting to justify. It is that for all least squares equations and their solutions, the variability, in a certain sense, that is not explained, is gotten from the sum of the 80 observations in Table XXa, each squared (229,057), the above estimates and the right-hand members of the equations. Then one takes advantage of the fact that the reduction in squares achieved by each estimate is given by the product of that estimate and the right-hand member of the equation from which it arises. Thus the reduction in variability, due to the introduction of $\hat{\mu}$, is -53.01(4241) as from Equ. (19), the reduction due to $\hat{\alpha}_1$ is -2.49(222) as from Equ. (24), the reduction due to $\hat{\beta}_1$ is -3.04(1121) as from Equ. (25). In total the variability residual on control variables (employing row and column estimates beyond those of Equ. (24) and (25) and not shown elsewhere) is:

$$\begin{aligned}
&229{,}057 - 53.01(4241) \\
&-\{+2.49(222) - .26(211) + 3.49(226) + 3.24(225) + 2.99(224) + 2.74(223) \\
&\quad +4.49(230) + 2.49(222) - 6.26(187) + 4.74(231) - 2.76(201) - 6.01(188) \\
&\quad -3.76(197) + 2.24(221) + .49(214) - 9.76(173) - 2.26(203) + 5.49(234) \\
&\quad -5.76(189) + 1.99(220)\} \\
&-\{+3.04(1121 - .61(1048) - 1.56(1029) - .86(1043)\} \\
&= 2507.91 \qquad (28)^{*}
\end{aligned}$$

This result is exact, except for rounding error, due to the fact that in the illustrative calculations there are very few decimal places. If the business is carried out with abundant decimal places the result of Equ. (28) is 2518.51, as in Table XXc.

Finally, we must consider the variability residual on the model when not only control factors, but also the experimental factors or Treatments, are included in the model for the data of Table XX . Since the effect of Treatments is orthogonal, on account of the peculiar nature of the Design, to those of Rows and Columns, i.e., the various equations are independent, we may write the further reduction due to the consideration of Treatments, after the fashion of Equ. (13) as subtracted directly from the previous residual to give a final residual of

---

*The reader should not be alarmed that some row estimates seem to give an increase, rather than a decrease, in residual variability. Matters are arranged so that Rows, as a whole, Columns, as a whole, and Treatments, as a whole, give each a reduction in variability.

$$2507.91-\{-1.01(1040)+.74(1075)+1.59(1092)-1.31(1034)\}=2381.07\ . \qquad (29)$$

or if abundant decimal places are used to the 2402.27 of Table XXc, where the results are summarized. There the mean reduction in variability per degree of freedom for Treatment is $(2518.51 - 240.27)/3 = 38.75$. The mean variability per degree of freedom residual to the entire model is $2402.27/54 = 44.49$. The ratio of these quantities is called $F = 38.75/44.49 = .87$, which is not significant on referral to Table L.

It should be realized, of course, that exactly the result gotten by the classic Equ. (15) could have been obtained by operations like those of Equ. (28) and (29). Equ. (15) is just a facilitation, convenient for a desk calculator, when one is dealing with a single complete latin square. It really does not matter how one analyzes--it is so easily done. It does not even matter much how one presents the analysis. Thus it can be presented in the classic way as in Table XIXc or in more concise but general manner as in XIXd. The approach is, however, important because the classical one becomes, often, unbearable in the more complicated cases that arise in the real world. Then the designs tend to be multiple, incomplete and not latin and the analysis had better be done by setting up the necessary equations and then considering the amount of variability left in the data after those equations have been used to make necessary estimates.

If one had only a desk calculator, one would use Equ. (10) through Equ. (15), with a small modification in the definition of S_T, to get the results shown in Table XX d. Generally in practice, however, research men would use the approach of Table XX c but take advantage of something like the appendix program. If one has access to an electronic computer it is much more simple just to give it the Design and results, as in Table XX a and have it yield the results as in Table XX c.

The writer suggests that this problem might more logically be thought of along the lines used in fitting a polynomial of indeterminate order, as Sir Ronald Fisher also taught us in 1925. Argue that there is a certain residual variability when allowance has been made for the control factors, i.e., Rows and Columns. Then consider the residual variability when allowance is further made for the Treatments. Consider the reduction in residual variability due to the addition of Treatments. Compare that reduction with the variability still residual to see whether the reduction due to Treatments is relatively so high that it could not be due to chance of the nature of the residual factors, i.e., are the particular Treatments concerned very meaningful. In all this handling about of residual variability it is necessary to bear in mind degrees of freedom, as set forth in Table XXc. We may say there were originally 80 degrees of freedom, i.e., 80 observations. There have been fitted constants, 1 for $\hat{\mu}$, $19 = 20 - 1$ for Rows and $3 = 4 - 1$ for Columns. This leaves $80 - 1 - 19 - 3 = 57$ d.f. associated with the residual squares of 2518.51 on the control factors. Now if the model is extended to include $3 = 4 - 1$ more constants for Treatments there are left only 54 d.f. with associated residual squares of 2402.27. There has been achieved a reduction in variability of

Table XX - Success in 20 Groups, using 4 Treatments each for 1 Week, during 4 Weeks

a. Design and results

	Week				
Group	1	2	3	4	Sum
I	(1) 53	(2) 58	(3) 58	(4) 53	222
V	(1) 57	(2) 55	(3) 54	(4) 45	211
IX	(1) 50	(2) 66	(3) 57	(4) 53	226
XIII	(1) 60	(2) 55	(3) 55	(4) 55	225
XVII	(1) 64	(2) 52	(3) 49	(4) 59	224
II	(2) 60	(4) 57	(1) 55	(3) 51	223
VI	(2) 64	(4) 51	(1) 53	(3) 62	230
X	(2) 53	(4) 58	(1) 51	(3) 60	222
XIV	(2) 55	(4) 44	(1) 48	(3) 40	187
XVIII	(2) 57	(4) 64	(1) 53	(3) 57	231
III	(3) 77	(1) 47	(4) 30	(2) 47	201
VII	(3) 44	(1) 49	(4) 48	(2) 47	188
XI	(3) 50	(1) 46	(4) 57	(2) 44	197
XV	(3) 63	(1) 51	(4) 52	(2) 55	221
XIX	(3) 62	(1) 42	(4) 56	(2) 54	214
IV	(4) 46	(3) 35	(2) 47	(1) 45	173
VIII	(4) 58	(3) 51	(2) 49	(1) 45	203
XII	(4) 54	(3) 63	(2) 51	(1) 66	234
XVI	(4) 42	(3) 49	(2) 51	(1) 47	189
XX	(4) 52	(3) 55	(2) 55	(1) 58	220
Sum	1121	1048	1029	1043	4241
Sum sq.	64,075	56,032	53,633	55,317	

b. Treatment estimates

	$(\hat{1})$	$(\hat{2})$	$(\hat{3})$	$(\hat{4})$
Contrib.	-1.01	+.74	+1.59	-1.31
Mean	52.00	53.75	54.60	51.70

c. Test of significance of Treatment effects

Factors	d.f.	Residual Variability (Squares)
$\hat{\mu}$, Rows & Columns (control)	57	2518.51
Control factors plus Treatments	54	2402.27

$$F = \frac{(2518.51 - 2402.27)/3}{2402.27/54} = .87 \text{ N.S.}$$

d. Testing of significance using classic analysis of variance

Source	d.f.	Sum Sq.	Mn. Sq.	F
Rows	19	1456.74	76.67	--
Columns	3	255.74	85.25	--
Treatments	3	116.24	38.75	.87 N.S.
Residuals	54	2402.27	44.49	
"Total"	79	4230.99		

2518.51 - 2402.27 = 116.24 (as it appears in the traditional XX d) for 3 d.f. or 38.75 per d.f. of Treatment. This is now to be contrasted with the 2402.27 residual variability for 54 d.f. or 44.49 per d.f. on the total model including control variables and Treatment. We contrast the 38.75 with 44.49 by striking the ratio of .87. Finally we decide if this is bigger than might be the case by accident by reference to Table L (5% point of F) to see the number 2.71. Our .87 is too small. If we had gotten 2.71 or more in our experiment we should have said the results were significant. Then what may seem curious subtraction above of one from number of Rows, Columns and Treatments is due to the condition, Equ. (9), which reduces the real number of things to be estimated. Thus, in Equ. (24), it seems that there are 20 values of $\hat{\alpha}_i$ to be estimated but there are really only 19 because $\hat{\alpha}_{20}$ is just the sum of $\hat{\alpha}_1$ through $\hat{\alpha}_{19}$, multiplied by minus one.

In the literature one can find the solemn handling of the control factors, say Rows and Columns, as if they were experimental Treatments, further elaborated for repeated latin squares. This position was first discussed in connection with the single latin square involved in Table XIX. The first 4 Groups, I, II, III and IV, may be regarded as belonging to a first square, the next 4 Groups, V, VI, VII and VIII, similarly belong to a second square. The Groups within a square may be more intimately related to one another than they are to other groups. In agricultural problems where the analogue of our Group is a Row of little experimental plots such is often the case. Then there is piled up in place of the single line lead by "Rows" in Table XX d an elaborate subdivision of variability due to squares, the interaction of squares with Columns etc. The reduction in variability (Sum Sq.) due to Treatments and the corresponding value for Residuals are completely unaffected and the F test remains unaffected. Accordingly, such elaboration seems generally to be an idle thing.

Breaking treatment variability up - Very often data such as those of Table VI are subject to general analysis as in Table XIXa but then certain particular comparisons are required. The variability among Treatments must be broken up. The point is that insofar as the Treatments make a substantial reduction in residual squares, i.e., 1585.7078 - 1074.9356 = 510.7722 it is because the treatment averages in Table XIXc vary substantially. It may be required to isolate that variability because there was some fundamental reason, known before the experiment was run, why certain comparisons should be made. There is often a situation where the variability among Treatments must be in some way subdivided. The Treatments are not simply so many alternate varieties, in themselves. Thus it may be required to compare Treatments (1) and (2), which are two types of regular Treatment, with the other Treatments as a set because they are experimental. Alternatively, it may be required to compare Treatments (2) and (3) with (4), (5) and (6) because the first 2 involved aureomycin and the latter 3 penicillin. The division for comparison may involve, in some way, all 6 Treatments, although it may involve the intercomparison of only some.

It is difficult and seems unnecessary, here, to go through the logic involved in such problems. We may advantageously avail ourselves of procedures well established in the analysis of variance. Consider there the comparison of any set A of Treatments compared with any other set B of Treatments. It is necessary to note the number of basic units

n_A and n_B in the two groups. In the case of a latin square, txtxt, as at present, n_A must be t times the number of Treatments in set A and n_B is similar. Then the sum of squares or mean square is

$$\frac{(T_A)^2}{n_A} + \frac{(T_B)^2}{n_B} - \frac{(T_A + T_B)^2}{n_A + n_B} \qquad (30)$$

where T_A is the total of all observations y in set A and T_B the total in set B . Such comparisons may be made as the nature of the problem involved indicate. All such mean squares have a unit freedom. The matter may be illustrated on two comparisons of Treatments in Table XIX, or Table VI, by calculating:

a. To compare Treatments (1) and (3) with all other Treatments find

$$\frac{(624.0)^2}{12} + \frac{(1407.8)^2}{24} - \frac{(2031.8)^2}{36} = 354.67 \quad . \qquad (31)$$

This mean square may be compared with the residual mean square of Table XIXd to give

$$F_{1,20} = 354.67/53.75 = 6.60 \quad , \qquad (32)$$

which on reference to Table L proves significant at the 5% level.

b. To compare $(\hat{2})$ and $(\hat{3})$ with $(\hat{4})$, $(\hat{5})$ and $(\hat{6})$ find

$$\frac{(646.8)^2}{12} + \frac{(1067.5)^2}{18} - \frac{(1714.3)^2}{30} = 210.38 \quad . \qquad (33)$$

This mean square when compared with residual gives

$$F_{1,20} = 210.38/53.75 = 3.91 \quad , \qquad (34)$$

which on reference to Table L is not significant at the 5% level.

This procedure may be paralleled by operations involving the estimates, as in Table XIXb, of Treatment effects. The procedure proposed is not exactly that usually given in discussions of the analysis of variance, although it is algebraically identical. It has considerable advantages in that it can be applied in a variety of situations without any special concepts or equations. It is, on the other hand, admittedly more clumsy for the case of the single, perfect latin square.

The set A , may be supposed to comprise m_A Treatments and set B to comprise m_B other Treatments. Then parallel to Equ. (30) we may write that the sum of squares or mean square for the comparison of the two sets is

$$E = K \left\{ \frac{(Z_A)^2}{m_A} + \frac{(Z_B)^2}{m_B} - \frac{(Z_A + Z_B)^2}{m_A + m_B} \right\} \quad (35)$$

where Z_A is the total of treatment estimates for set A and Z_B the total for set B . The constant K is to be found, as below. Of course, from comparison of Equ. (30) and (35) , for the single complete latin square, it is simply the number of experimental units under a given Treatment. In a more general way, K is the ratio of the reduction in residual squares due to Treatments, as in Table XIXa, to the sum of all the treatment estimates squared, i.e.,

$$(\hat{1})^2 + (\hat{2})^2 + (\hat{3})^2 + (\hat{4})^2 + (\hat{5})^2 + (\hat{6})^2 .$$

The matter is illustrated for the case of Table XIX in Equ. (36).

To illustrate the matter, consider the contributions of Table XIXb. We may find the value of

$$K = \frac{1585.7078 - 1074.9356}{(-3.52)^2 + (+.28)^2 + (-5.36)^2 + (+4.23)^2 + (-.69)^2 + (+5.06)^2} = 5.997 \qquad (36)$$

or, as previously discussed, actually 6, the number of observations under each Treatment, except for the fact that in the illustration the Treatment estimates, or contributions, are calculated with only two decimal places. Following Equ. (35), the part of the variability due to the comparison of set A, when it is Treatments (1) and (2) with set B, when it is the balance of the Treatments, is

$$K\left\{ \frac{\left((\hat{1}) + (\hat{3})\right)^2}{2} + \frac{\left((\hat{2}) + (\hat{4}) + (\hat{5}) + (\hat{6})\right)^2}{4} \right\}$$

when it will be noted that the third term of Equ. (35) vanishes because the numerator sums to zero. Numerically,

$$6\left\{ \frac{(-3.52 - 5.36)^2}{2} + \frac{(+.28 - .69 + 5.06 + 4.23)^2}{4} \right\}$$

$$= \frac{3}{4}(6)(8.88)^2$$

$$= 354.8448 . \qquad (37)$$

Now this value is to be compared with the final residual variability of Table XIXa to give

$$F_{1,20} = \frac{354.8448}{1074.9356/20} = 6.60 \qquad (38)$$

which on reference to Table L is significant at the 5% level. This result is, of course, identical with that of Equ. (32), except for rounding error. In the case of the comparison between Treatments (2) and (3) with (4), (5) and (6), the part of the variability due to this single comparison is

$$K\left\{\frac{\left((\hat{2})+(\hat{3})\right)^2}{2}+\frac{\left((\hat{4})+(\hat{5})+(\hat{6})\right)^2}{3}-\frac{\left((\hat{2})+(\hat{3})+(\hat{4})+(\hat{5})+(\hat{6})\right)^2}{5}\right\}, \quad (39)$$

i.e., there is a third "corrective" term within the complex brackets. Thus from Table XIXb, again, the comparison yields

$$6\left\{\frac{(+.28-5.36)^2}{2}+\frac{(+4.23-.69+5.06)^2}{3}-\frac{(+.28-5.36+4.23-.69+5.06)^2}{5}\right\}$$

$$= 210.4707 . \quad (40)$$

Now this value is to be compared with the final residual variability of Table XIXa to give

$$F_{1,20}=\frac{210.4707}{1074.9356/20}=3.92 \quad (41)$$

which on reference to Table L proves not significant. This result is, of course, identical with that of Equ. (34), except for rounding error.

In actual experimentation, there frequently arises the problem where some one picks out, after the fact, of Table XIXb the most divergent Treatment, like $(\hat{3})$ at -5.36, and compares it with the balance of the Treatments. The calculation is simple enough, being along the lines previously followed to give

$$F_{1,20} = \frac{6(6)(5.36)^2(20)}{1074.9356(5)}$$

$$= 3.85 \quad . \tag{42}$$

There arises, however, a problem of testing this value for significance. It is a problem in logic. The quantity $F_{1,20}$ cannot be referred in a simple way to Table L if we deliberately chose 5.36 on account of its being large; we did not predesignate it. There is a considerable discussion on this matter in the literature although it will be found to center around the quantity $\sqrt{F_{1,20}}$ which is called "Student's." (The choice of symbols has no great significance but is an historic accident.) To anyone interested in this question we recommend a search of the literature on this logical problem.

There are subdivisions of the variability among Treatments, other than the comparisons just detailed. For instance there are factorial experiments where 3 chemicals are tried each at 2 concentrations to produce 6 Treatments. One may want, as it is said, to study the effects of the factor of kind of reagent, the factor of concentration and possible interaction of these two factors. Such matters are covered in all standard discussions of experimentation and should not be allowed to divert us from more immediate business. Suffice it to say that in such discussions the sums given may be judiciously replaced by estimates and then the value K used to adjust matters.

Analysis with allowance for Carry-over in latin squares with conditioning Period - In Chap. II, as in connection with Table VI, it was suggested that Carry-over of Treatment effects may often be considerable. On this basis the Designs presented and discussed have made allowance for Change-over. That is a given Treatment is not preceded by any other Treatment more than once. With such Designs we may at least comfort ourselves that Carry-over, if it occurs, will be confounded in a small and presumably minimal way with Treatment effects. The matter often stops there. It may, however, be necessary to free treatment effects from Carry-over. It may further be required to get Carry-over out of the residual mean square; Carry-over blows this up. There are even times when it is of great practical importance to estimate the effect of Change-over. Thus we are often under the necessity of analyzing for Change-over. Consider first the situation when the Design contains a conditioning Period, as in Table VI, and as reproduced in Table XXIa, so that there are Carries-over in the first Period of the experiment, proper, just as in the subsequent Periods.

It is necessary to extend, for Columns after the first, the more usual model for a latin square as given in Equ. (7) to

$$y_{ijk\ell} = \mu + \alpha_i + \beta_j + \gamma_k + \delta_\ell + \varepsilon_{ijk\ell} \qquad (43)$$

where the i^{th} Row, j^{th} Column, k^{th} Treatment, and ℓ^{th} Carry-over are involved with extraneous variability, $\varepsilon_{ijk\ell}$, of some kind. It is supposed, as in Equ. (9),

$$\sum_{i=1}^{t} \delta_\ell = 0 \ . \qquad (44)$$

Thus an observation y_{2364} is conceived as being like y_{236} of Equ. (8) except that we recognize, additionally, that the 9th result 64.2 followed Treatment (4), i.e., possibly contains Carry-over, δ_4. The variability extraneous to the effect of all four factors in this particular cell is $\varepsilon_{ijk\ell}$. It is now possible to build again estimates of the various effects and the amount of variability residual on them. There does arise a question as to whether we are trying to test the significance of carry-over effects or simply to eliminate them in the same way as the effect of Rows and Columns. Then, of course, we may do, and shall do in the following discussion, both, one at a time. Again, it must be noted that the following discussion of Carry-over is based upon the additive model of Equ. (43). In this it parallels the traditional analysis of variance built on the same sort of assumption. There is perhaps a little more reason to question the assumption in the more elaborate model but it will not be done in the present book.

It may be now observed that there is a reward for the primitive approach to the writing of least squares equations as set forth previously in connection with the multiple latin square, without consideration of possible Carry-over. There are no well-known solutions for the present effects. The procedure in the literature involves the calculation of an elaborate set of preliminary quantities and their labyrinthine manipulation. Our reasonable but uninstructed man would simply form the obvious and appropriate totals and get exactly the right effects. Thus he would argue that the sum of all the observations $y_{ijk\ell}$ would contain μ 36 times and all other row, column, treatment and carry-over effects in such balance that they cancelled and find

$$36\hat{\mu} = 2031.8 \tag{45}$$

like Equ. (19), where the argument is more detailed, because in total all other effects balance out and disappear by virtue of Equ. (9) and Equ. (44). Incidentally,

$$\hat{\mu} = 56.44 \quad . \tag{46}$$

What is more important is to gather up Equ. (45) as the first line in Equ. (48) together with other equations. In order to form an estimate of effect of Group I he would note that the sum of the observations comprises in the terms of Equ. (43),

$$\begin{aligned} &6\hat{\mu} + 6\hat{\alpha}_1 + (\hat{\beta}_1 + \hat{\beta}_2 + \hat{\beta}_3 + \hat{\beta}_4 + \hat{\beta}_5 + \hat{\beta}_6) + (\hat{\gamma}_1 + \hat{\gamma}_2 + \hat{\gamma}_3 + \hat{\gamma}_4 + \hat{\gamma}_5 + \hat{\gamma}_6) \\ &\qquad + (2\hat{\delta}_1 + \hat{\delta}_2 + \hat{\delta}_3 + \hat{\delta}_4 + \hat{\delta}_5) \\ &= 6\hat{\mu} + 6\hat{\alpha}_1 + \hat{\delta}_1 - \hat{\delta}_6 \\ &= 317.0 \quad . \end{aligned} \tag{47}$$

In a similar way he might set up totals for all values of $y_{ijk\ell}$ within Row of the Design, to produce lines 2 through 7 of Equ. (48). Actually, the 7th has to be replaced from Equ. (9) because if one were to treat the first 7 lines as simultaneous equations one would find what is called a singularity, i.e., they would not solve, but the matter can be resolved by this substitution. This problem did not arise previously when all the equations were simple, but now Carry-over is, as the word goes, confounded with row effects, in Equ. (48). Similarly, the Columns yield 6 more equations, including one from Equ. (9). Treatments yield 6 more. Carries-over yield 6 more although there the substitution is from Equ. (44). These are least squares equations.

$$\left.\begin{array}{ccccccccccccccccccccccccccr}
\hat\mu & \hat\alpha_1 & \hat\alpha_2 & \hat\alpha_3 & \hat\alpha_4 & \hat\alpha_5 & \hat\alpha_6 & \hat\beta_1 & \hat\beta_2 & \hat\beta_3 & \hat\beta_4 & \hat\beta_5 & \hat\beta_6 & \hat\gamma_1 & \hat\gamma_2 & \hat\gamma_3 & \hat\gamma_4 & \hat\gamma_5 & \hat\gamma_6 & \hat\delta_1 & \hat\delta_2 & \hat\delta_3 & \hat\delta_4 & \hat\delta_5 & \hat\delta_6 & = & \text{Const.} \\
\hline
+36 & & & & & & & & & & & & & & & & & & & & & & & & & = & 2031.8 \\
+6 & +6 & & & & & & & & & & & & & & & & & & +1 & & & -1 & & & = & 317.0 \\
+6 & & +6 & & & & & & & & & & & & & & & & & & +1 & & & -1 & & = & 336.5 \\
+6 & & & +6 & & & & & & & & & & & & & & & & & & +1 & & & -1 & = & 336.8 \\
+6 & & & & +6 & & & & & & & & & & & & & & & -1 & & & +1 & & & = & 370.7 \\
+6 & & & & & +6 & & & & & & & & & & & & & & & -1 & & & +1 & & = & 344.3 \\
 & +1 & +1 & +1 & +1 & +1 & +1 & & & & & & & & & & & & & & & & & & & = & .0 \\
+6 & & & & & & & +6 & & & & & & & & & & & & & & & & & & = & 333.0 \\
+6 & & & & & & & & +6 & & & & & & & & & & & & & & & & & = & 336.1 \\
+6 & & & & & & & & & +6 & & & & & & & & & & & & & & & & = & 329.8 \\
+6 & & & & & & & & & & +6 & & & & & & & & & & & & & & & = & 353.3 \\
+6 & & & & & & & & & & & +6 & & & & & & & & & & & & & & = & 342.1 \\
 & & & & & & & +1 & +1 & +1 & +1 & +1 & +1 & & & & & & & & & & & & & = & .0 \\
+6 & & & & & & & & & & & & & +6 & & & & & & & & & & & & = & 317.5 \\
+6 & & & & & & & & & & & & & & +6 & & & & & & & & & & & = & 340.3 \\
+6 & & & & & & & & & & & & & & & +6 & & & & & & & & & & = & 306.5 \\
+6 & & & & & & & & & & & & & & & & +6 & & & & & & & & & = & 364.0 \\
+6 & & & & & & & & & & & & & & & & & +6 & & & & & & & & = & 334.5 \\
 & & & & & & & & & & & & & +1 & +1 & +1 & +1 & +1 & +1 & & & & & & & = & .0 \\
+6 & +1 & & & -1 & & & & & & & & & & & & & & & +6 & & & & & & = & 308.6 \\
+6 & & +1 & & & -1 & & & & & & & & & & & & & & & +6 & & & & & = & 340.9 \\
+6 & & & +1 & & & -1 & & & & & & & & & & & & & & & +6 & & & & = & 323.3 \\
+6 & -1 & & & +1 & & & & & & & & & & & & & & & & & & +6 & & & = & 355.3 \\
+6 & & -1 & & & +1 & & & & & & & & & & & & & & & & & & +6 & & = & 334.2 \\
 & & & & & & & & & & & & & & & & & & & +1 & +1 & +1 & +1 & +1 & +1 & = & .0
\end{array}\right\}(48)$$

(The positions left empty contain zeroes, not shown)

In a general way it is first necessary to test for the effect of Treatments when Carry-over is uninvolved in the model, as was previously done in connection with Table XIX. Now, however, the matter will be approached from the point of view of the least squares equations. The basic model will be that of Equ. (43) but for the moment the elements of Treatment and Carry-over will be neglected. In a general way the columns (14th through 25th), involving $\hat{\gamma}_k$ and $\hat{\delta}_\ell$ of Equ. (48) and the corresponding rows must be dropped. This is a matter of eliminating the control factors. In detail, the equations are:

$$\left.\begin{array}{ccccccccccccccc}
\hat{\mu} & \hat{\alpha}_1 & \hat{\alpha}_2 & \hat{\alpha}_3 & \hat{\alpha}_4 & \hat{\alpha}_5 & \hat{\alpha}_6 & \hat{\beta}_1 & \hat{\beta}_2 & \hat{\beta}_3 & \hat{\beta}_4 & \hat{\beta}_5 & \hat{\beta}_6 & = & \text{Sum} \\
\hline
+36 & & & & & & & & & & & & & = & 2031.8 \\
+6 & +6 & & & & & & & & & & & & = & 317.0 \\
+6 & & +6 & & & & & & & & & & & = & 336.5 \\
+6 & & & +6 & & & & & & & & & & = & 336.8 \\
+6 & & & & +6 & & & & & & & & & = & 370.7 \\
+6 & & & & & +6 & & & & & & & & = & 344.3 \\
 & +1 & +1 & +1 & +1 & +1 & +1 & & & & & & & = & .0 \\
+6 & & & & & & & +6 & & & & & & = & 333.0 \\
+6 & & & & & & & & +6 & & & & & = & 336.1 \\
+6 & & & & & & & & & +6 & & & & = & 329.8 \\
+6 & & & & & & & & & & +6 & & & = & 353.3 \\
+6 & & & & & & & & & & & +6 & & = & 342.1 \\
 & & & & & & & +1 & +1 & +1 & +1 & +1 & +1 & = & .0
\end{array}\right\} \qquad (49)$$

(The positions left empty contain zeroes, not shown)

The resultant estimates are:

$$\left.\begin{array}{cccccc}
\hat{\alpha}_1 & \hat{\alpha}_2 & \hat{\alpha}_3 & \hat{\alpha}_4 & \hat{\alpha}_5 & \hat{\alpha}_6 \\
\hline
-3.61 & -.36 & -.31 & +5.34 & +.94 & -2.02 \\
\hat{\beta}_1 & \hat{\beta}_2 & \hat{\beta}_3 & \hat{\beta}_4 & \hat{\beta}_5 & \hat{\beta}_6 \\
\hline
-.94 & -.42 & -1.47 & +2.44 & +.58 & -.19
\end{array}\right\} . \qquad (50)$$

It now is necessary to find the residual squares on estimates of $\hat{\mu}$, Rows and Columns as control factors. This requires an operation similar to that of Equ. (28). This residual variability is:

$$
\begin{aligned}
&116{,}596.26 - 56.44(2031.8) \\
&\quad -\{-3.61(317.0) -.36(336.5) -.31(336.8) +5.34(370.7) +.94(344.3) -2.02(326.5)\} \\
&\quad -\{-.94(333.0) -.42(336.1) -1.47(329.8) +2.44(353.3) +.58(342.1) -.19(337.5)\} \\
&\qquad = 1590.38 \qquad\qquad (51)
\end{aligned}
$$

or if the thing be carried out with 10-figure accuracy, of the estimates, so that the results gibe with those of Table XIXa, 1585.7078. In order to find the further reduction due to Treatments, from Equ. (48) the columns (20th through 25th) involving $\hat{\delta}_\ell$ and the corresponding rows must be dropped. In detail, the equations are:

$$
\left.
\begin{array}{*{20}{c}r}
\hat{\mu} & \hat{\alpha}_1 & \hat{\alpha}_2 & \hat{\alpha}_3 & \hat{\alpha}_4 & \hat{\alpha}_5 & \hat{\alpha}_6 & \hat{\beta}_1 & \hat{\beta}_2 & \hat{\beta}_3 & \hat{\beta}_4 & \hat{\beta}_5 & \hat{\beta}_6 & \hat{\gamma}_1 & \hat{\gamma}_2 & \hat{\gamma}_3 & \hat{\gamma}_4 & \hat{\gamma}_5 & \hat{\gamma}_6 & = & \text{Sum} \\
\hline
+36 &&&&& &&&&& &&&&& &&&& = & 2031.8 \\
+6 & +6 &&&&& &&&&& &&&&& &&& = & 317.0 \\
+6 && +6 &&&&& &&&&& &&&&& && = & 336.5 \\
+6 &&& +6 &&&&& &&&&& &&&&& & = & 336.8 \\
+6 &&&& +6 &&&&& &&&&& &&&&& = & 370.7 \\
+6 &&&&& +6 &&&&& &&&&& &&&& = & 344.3 \\
& +1 & +1 & +1 & +1 & +1 & +1 &&&&& &&&&& &&& = & .0 \\
+6 &&&&& && +6 &&&&& &&&&& && = & 333.0 \\
+6 &&&&& &&& +6 &&&&& &&&&& & = & 336.1 \\
+6 &&&&& &&&& +6 &&&&& &&&&& = & 329.8 \\
+6 &&&&& &&&&& +6 &&&&& &&&& = & 353.3 \\
+6 &&&&& &&&&& & +6 &&&&& &&& = & 342.1 \\
&&&&& && +1 & +1 & +1 & +1 & +1 & +1 &&&&& && = & .0 \\
+6 &&&&& &&&&& &&& +6 &&&&& & = & 317.5 \\
+6 &&&&& &&&&& &&&& +6 &&&&& = & 340.3 \\
+6 &&&&& &&&&& &&&&& +6 &&&& = & 306.5 \\
+6 &&&&& &&&&& &&&&& & +6 &&& = & 364.0 \\
+6 &&&&& &&&&& &&&&& && +6 && = & 334.5 \\
&&&&& &&&&& &&& +1 & +1 & +1 & +1 & +1 & +1 & = & .0
\end{array}
\right\}
(52)
$$

(The positions left empty contain zeroes, not shown)

The resultant estimates for $\hat{\mu}$, $\hat{\alpha}_i$ and $\hat{\beta}_j$ are as in Equ. (46)

and (50). Those for $\hat{\gamma}_k$ which remain as previously from Equ. (12) are shown in Table XIXb and XXIc. The residual variability is:

$$\begin{aligned}
&116{,}596.26 - 56.44(2031.8)\\
&\quad -\{-3.61(317.0) -.36(336.5) -.31(336.8) +5.34(370.7) +.94(344.3) -2.02(326.5)\}\\
&\quad -\{-.94(333.0) -.42(336.1) -1.47(329.8) +2.44(353.3) +.58(342.1) -.19(337.5)\}\\
&\quad -\{-3.52(317.5)+.28(340.3) -5.36(306.5) +5.06(369.0) +4.23(364.0) -.69(334.5)\}\\
&= 1590.38\\
&\quad -\{-3.52(317.5)+.28(340.3) -5.36(306.5) +5.06(369.0) +4.23(364.0) -.69(334.5)\}\\
&\qquad = 1079.48 \qquad\qquad (53)
\end{aligned}$$

or if the thing be carried out with 10-figure accuracy, of the estimates, so that the results give with those of Table XIXa, 1074.9356. These results are summarized in Table XXIb; the value F fails significance.

As a detail, in actual computer operation, both here and subsequently, the reduction in matrix, as from Equ. (48) to (52), is most easily and accurately accomplished by submitting the first 19 lines, or equations, as they are, replacing the 20th through 25th lines or equations by $\hat{\delta}_\ell = 0$, for all ℓ. If, for instance, each line is a punched card, the last 6 cards have to be temporarily replaced. The last corresponding 6 columns automatically become ineffectual. Thus in the present case, rather than Equ. (52), one might submit the equivalent,

$$\left.\begin{array}{*{26}{c}r}
\hat{\mu} & \hat{\alpha}_1 & \hat{\alpha}_2 & \hat{\alpha}_3 & \hat{\alpha}_4 & \hat{\alpha}_5 & \hat{\alpha}_6 & \hat{\beta}_1 & \hat{\beta}_2 & \hat{\beta}_3 & \hat{\beta}_4 & \hat{\beta}_5 & \hat{\beta}_6 & \hat{\gamma}_1 & \hat{\gamma}_2 & \hat{\gamma}_3 & \hat{\gamma}_4 & \hat{\gamma}_5 & \hat{\gamma}_6 & \hat{\delta}_1 & \hat{\delta}_2 & \hat{\delta}_3 & \hat{\delta}_4 & \hat{\delta}_5 & \hat{\delta}_6 & = & \text{Const.} \\
\hline
+36 &&&&&& &&&&&& &&&&&& &&&&&& & = & 2031.8 \\
+6 & +6 &&&&& &&&&&& &&&&&& & +1 &&& -1 && & = & 317.0 \\
+6 && +6 &&&& &&&&&& &&&&&& && +1 &&& -1 & & = & 336.5 \\
+6 &&& +6 &&& &&&&&& &&&&&& &&& +1 &&& -1 & = & 336.8 \\
+6 &&&& +6 && &&&&&& &&&&&& & -1 &&& +1 && & = & 370.7 \\
+6 &&&&& +6 & &&&&&& &&&&&& && -1 &&& +1 & & = & 344.3 \\
 & +1 & +1 & +1 & +1 & +1 & +1 &&&&&& &&&&&& &&&&&& & = & .0 \\
+6 &&&&&& & +6 &&&&& &&&&&& &&&&&& & = & 333.0 \\
+6 &&&&&& && +6 &&&& &&&&&& &&&&&& & = & 336.1 \\
+6 &&&&&& &&& +6 &&& &&&&&& &&&&&& & = & 329.8 \\
+6 &&&&&& &&&& +6 && &&&&&& &&&&&& & = & 353.3 \\
+6 &&&&&& &&&&& +6 & &&&&&& &&&&&& & = & 342.1 \\
 &&&&&& & +1 & +1 & +1 & +1 & +1 & +1 &&&&&& &&&&&& & = & .0 \\
+6 &&&&&& &&&&&& & +6 &&&&& &&&&&& & = & 317.5 \\
+6 &&&&&& &&&&&& && +6 &&&& &&&&&& & = & 340.3 \\
+6 &&&&&& &&&&&& &&& +6 &&& &&&&&& & = & 306.5 \\
+6 &&&&&& &&&&&& &&&& +6 && &&&&&& & = & 364.0 \\
+6 &&&&&& &&&&&& &&&&& +6 & &&&&&& & = & 334.5 \\
 &&&&&& &&&&&& & +1 & +1 & +1 & +1 & +1 & +1 &&&&&& & = & .0 \\
 &&&&&& &&&&&& &&&&&& & +1 &&&&& & = & .0 \\
 &&&&&& &&&&&& &&&&&& && +1 &&&& & = & .0 \\
 &&&&&& &&&&&& &&&&&& &&& +1 &&& & = & .0 \\
 &&&&&& &&&&&& &&&&&& &&&& +1 && & = & .0 \\
 &&&&&& &&&&&& &&&&&& &&&&& +1 & & = & .0 \\
 &&&&&& &&&&&& &&&&&& &&&&&& +1 & = & .0
\end{array}\right\} (54)$$

(The positions left empty contain zeroes, not shown)

It is, secondly, necessary to test for the effect of Treatments when Carry-over is involved, i.e., on the full model of Equ. (43). In order to do this we must find the estimates and residual variability when Treatment is not included and then when it is included. In a general way the columns (14th through 19th) involving $\hat{\gamma}_k$ and corresponding rows of Equ. (48) must be dropped. We are making an analysis on the basis of estimating Carry-over as a control factor, like Rows and Columns. In detail, the equations are:

$$\left.\begin{array}{ccccccccccccccccccccr}
\hat{\mu} & \hat{\alpha}_1 & \hat{\alpha}_2 & \hat{\alpha}_3 & \hat{\alpha}_4 & \hat{\alpha}_5 & \hat{\alpha}_6 & \hat{\beta}_1 & \hat{\beta}_2 & \hat{\beta}_3 & \hat{\beta}_4 & \hat{\beta}_5 & \hat{\beta}_6 & \hat{\delta}_1 & \hat{\delta}_2 & \hat{\delta}_3 & \hat{\delta}_4 & \hat{\delta}_5 & \hat{\delta}_6 & = & \text{Sum} \\
\hline
+36 & & & & & & & & & & & & & & & & & & & = & 2031.8 \\
+6 & +6 & & & & & & & & & & & & +1 & & & -1 & & & = & 317.0 \\
+6 & & +6 & & & & & & & & & & & & +1 & & & -1 & & = & 336.5 \\
+6 & & & +6 & & & & & & & & & & & & +1 & & & -1 & = & 336.8 \\
+6 & & & & +6 & & & & & & & & & -1 & & & +1 & & & = & 370.7 \\
+6 & & & & & +6 & & & & & & & & & -1 & & & +1 & & = & 344.3 \\
 & +1 & +1 & +1 & +1 & +1 & +1 & & & & & & & & & & & & & = & .0 \\
+6 & & & & & & & +6 & & & & & & & & & & & & = & 333.0 \\
+6 & & & & & & & & +6 & & & & & & & & & & & = & 336.1 \\
+6 & & & & & & & & & +6 & & & & & & & & & & = & 329.8 \\
+6 & & & & & & & & & & +6 & & & & & & & & & = & 353.3 \\
+6 & & & & & & & & & & & +6 & & & & & & & & = & 342.1 \\
 & & & & & & & +1 & +1 & +1 & +1 & +1 & +1 & & & & & & & = & .0 \\
+6 & +1 & & & -1 & & & & & & & & & +6 & & & & & & = & 308.6 \\
+6 & & +1 & & & -1 & & & & & & & & & +6 & & & & & = & 340.9 \\
+6 & & & +1 & & & -1 & & & & & & & & & +6 & & & & = & 323.3 \\
+6 & -1 & & & +1 & & & & & & & & & & & & +6 & & & = & 355.3 \\
+6 & & -1 & & & +1 & & & & & & & & & & & & +6 & & = & 334.2 \\
 & & & & & & & & & & & & & +1 & +1 & +1 & +1 & +1 & +1 & = & .0
\end{array}\right\} \quad (55)$$

(The positions left empty contain zeroes, not shown)

The resultant estimates are:

$$\begin{array}{cccccc}
\hat{\alpha}_1 & \hat{\alpha}_2 & \hat{\alpha}_3 & \hat{\alpha}_4 & \hat{\alpha}_5 & \hat{\alpha}_6 \\
\hline
-2.71 & -.65 & +1.25 & +4.44 & +1.24 & -3.57
\end{array} \quad (56)$$

which differ from those of Equ. (50) because the estimates of $\hat{\delta}_\ell$, or

Carry-over, have become involved while the estimates of β_j remain as there because Carry-over is uninvolved in Column estimates. The estimates for $\hat{\delta}_\ell$ or $[\hat{\ell}]$ are as shown in Table XXIc. It is now necessary to find the residual squares on estimates of $\hat{\mu}$, Rows, Columns and Carry-over as control factors. This requires an operation similar to that of Equ. (28). The residual variability is:

$$
\begin{aligned}
&116{,}596.26 - 56.44(2031.8) \\
&-\{-2.71(317.0) - .65(336.5) + 1.25(336.8) + 4.44(370.7) + 1.24(344.3) - 3.57(326.5)\} \\
&-\{-.94(333.0) - .42(336.1) - 1.47(329.8) + 2.44(353.3) + .58(342.1) - .19(337.5)\} \\
&-\{-3.82(308.6) + .69(340.9) - 3.36(323.3) + 1.59(355.3) - 1.05(334.2) + 5.95(369.5)\} \\
&= 1231.05 \qquad (57)
\end{aligned}
$$

or if the thing be carried out with higher accuracy, of the estimates, 1233.5020. In order to find the further reduction due to Treatments it is necessary to consider their estimates which remain as previously from Equ. (12) and shown in Table XIXb, since Treatments are confounded with neither Rows nor Carries-over, in this Design. Accordingly one must make a further subtraction, like that of Equ. (53), from the result of Equ. (57), as:

$$
\begin{aligned}
&1231.05 \\
&\quad -\{-3.52(317.5) + .28(340.3) - 5.36(306.5) + 5.06(369.0) + 4.23(364.0) - .69(334.5)\} \\
&\quad = 720.15 \qquad (58)
\end{aligned}
$$

or if the thing be carried out with higher accuracy, of the estimates, 722.7298. These results are summarized in Table XXId; the value F fails significance, although it is a little greater than the F in Table XIX--the introduction of Carry-over into the model is responsible for this.

The full model of Equ. (48), together with the various reduced parts of those equations, as previously discussed will in actual practice be handled, automatically, by the program, for electronic computer, in the Appendix.

The procedure used in the present section of fitting models at successive levels of complexity, e.g., up to Carry-over and then with the addition of direct treatment effects is, as was remarked previously in connection with multiple latin squares, on the general principle familiar in fitting polynomials of increasing complexity. There one may fit a parabola and then see if any substantial improvement results if one goes on to the complexity of a cubic. The only difference is that in the present work one adds necessarily a whole cluster of constants instead of a single constant.

The alternative analysis for significance of Carry-over will not usually be of practical importance; Carry-over is a means to an end, it is just a control factor like Rows and Columns and it is surely of little interest to test their significance. The results are shown in Table XXIe. The first line comes directly from the second line of Table XIXa. The second line directly from the second line of Table XXIb.

Table XXI - Analysis of latin square with conditioning Period for direct Treatment and Carry-over

a. Data collected, repeated from Table VI

		Week							
Group	0	1	2	3	4	5	6	Sum	Mean
I	((1))	(1)46.4	(3)45.8	(2)40.8	(5)62.4	(6)59.9	(4)61.7	317.0	52.8
II	((2))	(2)60.9	(4)59.2	(3)44.9	(6)64.2	(1)55.3	(5)52.0	336.5	56.1
III	((3))	(3)50.0	(5)50.0	(4)64.2	(1)60.9	(2)58.4	(6)53.3	336.8	56.1
IV	((4))	(4)63.7	(6)72.0	(5)71.7	(2)57.3	(3)53.3	(1)52.7	370.7	61.8
V	((5))	(5)48.8	(1)50.4	(6)56.4	(3)58.9	(4)65.6	(2)64.2	344.3	57.4
VI	((6))	(6)63.2	(2)58.7	(1)51.8	(4)49.6	(5)49.6	(3)53.6	326.5	54.4
Sum		333.0	336.1	329.8	353.3	342.1	337.5	2031.8	
Mean		55.5	56.0	55.0	58.9	57.0	56.2		56.4
Sum, Sq.		18,795.14	19,272.13	18,807.38	20,936.87	19,661.07	19,123.67		

b. Analysis for direct Treatment alone (reproduced from Table XXc)

Factors	d.f.	Residual Variability (Squares)
$\hat{\mu}$, Rows & Columns (control)	25	1585.7078
Control factors plus Treatment	20	1074.9356

$$F_{5,20} = \frac{(1585.7078 - 1074.9356)/(25-20)}{1074.9356/20} = 1.90 \text{ N.S.}$$

c. Direct Treatment effects and Carry-over effects

	$(\hat{1})$	$(\hat{2})$	$(\hat{3})$	$(\hat{4})$	$(\hat{5})$	$(\hat{6})$
Contrib.	-3.52	+.28	-5.36	+4.23	-.69	+5.06
Mean	52.92	56.72	51.08	60.67	55.75	61.50

$[\hat{1}]$	$[\hat{2}]$	$[\hat{3}]$	$[\hat{4}]$	$[\hat{5}]$	$[\hat{6}]$
-3.82	+.69	-3.36	+1.59	-1.05	+5.95

d. Significance of direct Treatment effects

Factors	d.f.	Residual Variability (Squares)
$\hat{\mu}$, Rows, Columns, Carries-over (control)	20	1233.5020
Control factors plus Treatments	15	722.7298

$$F_{5,15} = \frac{(1233.5020 - 722.7298)/5}{722.7298/15} = 2.12 \text{ N.S.}$$

e. Significance of Carry-over effects

Factors	d.f.	Residual Variability (Squares)
$\hat{\mu}$, Rows, Columns, Treatments (control)	20	1074.9356
Control factors plus Carries-over	15	722.7298

$$F_{5,15} = \frac{(1074.9356 - 722.7298)/5}{722.7298/15} = 1.46 \text{ N.S.}$$

f. Estimation of direct Treatment and Carry-over in a simple way by neglecting Row effect (reproduced from Table VIb)

	Treatment								
After	(1)	(2)	(3)	(4)	(5)	(6)	Sum	Mean	Contrib
(1)	46.4	58.4	45.8	49.6	52.0	56.4	308.6	51.4	-5.0
(2)	51.8	60.9	53.3	59.2	62.4	53.3	340.9	56.8	+ .4
(3)	52.7	40.8	50.0	65.6	50.0	64.2	323.3	53.9	-2.5
(4)	60.9	64.2	44.9	63.7	49.6	72.0	355.3	59.2	+2.8
(5)	50.4	57.3	53.6	64.2	48.8	59.9	334.2	55.7	- .7
(6)	55.3	58.7	58.9	61.7	71.7	63.2	369.5	61.6	+5.2
Sum	317.5	340.3	306.5	364.0	334.5	369.0	2031.8		
Mean	52.9	56.7	51.1	60.7	55.8	61.5		56.4	
Contrib.	-3.5	+.3	-5.3	+4.3	-.6	+5.1			

The residual variability divided by its degrees of freedom may be termed the mean residual square. Thus in Table XXIb it is 1074.9356/20 = 53.75 whereas in XXId it is 722.7298/15 = 48.18. In either case it is related to the amount of inconsistency in the data--inconsistency in the sense that the variability of the data can be explained by neither the control factors nor Treatment. The two values are fairly strictly comparable. The smaller the value the better the model--if it were zero the model of control factors and Treatments would be perfect. The concept is often useful in studying the results from an experiment. There is a somewhat related idea of mean square for Treatment such as (1585.7078 - 1074.9356)/5 = 102.15. This is commonly found in the classic analysis of variance. It seems to be of little use and the root of much conceptual difficulty. It is the sort of thing best not thought about.

In connection with Table XXI , when allowance is made for Carry-over, note that a quantity called mean residual variability, i.e., terminal residual (squares) variability divided by the appropriate degrees of freedom, or the quantity that appears in the denominator of F , may be a good bit the less than when no such allowance is made. The reduction is less than might have been expected but goes from 53.75 in Table XIX to 48.18, i.e., is 10% less. In much change-over material that comes to hand it turns out that much more of the variability that we should otherwise consider residual is, in fact, Carry-over. Insofar as this happens something that previously passed for extraneous or chance variation is, in fact, assigned to Carry-over. This affects the significance of the Treatments, proper. The effect of Treatments which was not previously significant then becomes significant. In a formal way, the denominator of the F ratio is reduced and so the significance is increased. In a more real way, we judge the treatment responses much more consistent and subject to far less variability due to extraneous factors. In a general way, for many people, failure to allow in analysis of their experiments for Carry-over must exaggerate the extraneous or chance variation and often lead them falsely to the conclusion that their experimental effects are not significant.

The correlation, +.93, between Carry-over and direct Treatment effect in Table XXIc is considerable. In such cases one feels there must have been some real treatment effects, for how could they otherwise carry-over? Such evidence seems almost more convincing than the familiar test that the direct treatment effects contribute significantly to the reduction in residual variability. Incidentally, it may be noted that this value for the correlation was found previously in connection with Table VI between approximate Carries-over and Treatments. This approximate work is reported in Table XXIf.

Table XXI is concluded by the simple estimation of direct treatment effects and Carry-over by neglecting row effect, as it was first done in Table VI. This may be a useful device for a man with limited calculating facilities. It will be noted that the treatment effects are exactly correct, by comparing Table XXIc and XXIb. This is as one would expect since in this Design they are, as previously, unconfounded with Rows or Carries-over. The Carries-over agree reasonably well with those gotten exactly in Table XXIc. Any discrepancy is due to the fact that in Table XXIb there is no adjustment for row effects which do enter, as in Equ. (54).

In the present case the correlation between these rough estimates and exact estimates is .97. Accordingly, it is possible to get a quick idea of whether there seems to be Carry-over present in data and the ill-provided man might direct his analysis on this basis. It may be of interest to note that the operation in Table XXIf is, in terms of the general Equ. (48) equivalent to setting $\hat{\alpha}_i = 0$, for all i. This is essentially an analysis based on a model with Columns, Treatments and Carries-over, but no allowance for Rows--thus is it simplified.

Carry-over is often of positive nature in the work used in this book for illustration. Sometimes, however, under similar conditions it may be negative, or at least neutral. To illustrate this point there are shown the data of Table XXII on material similar to that of Table XXI. The Design is identical and hence the analysis; only the nature of the results is different.

The correlation between Carry-over and direct treatment effect, using their estimates as in Table XXIIc, is -.82 which is sizeable. It is a little difficult to judge its significance; the 5% level for 6 independent variables is -.81. The present estimates are not, of course, independent and it would be arduous to find the strict level of significance. As was argued previously, in connection with Table XXI, one must, however, feel there to have been some real treatment effects, insofar as they did carry-over. Such evidence seems, again, almost more convincing than the familiar test that the direct treatment effects contribute significantly to the reduction in residual variability. On the other hand, it must be admitted that the introduction of Carry-over to the model did not advantage the test of significance for Treatments proper. The value of F dropped from 5.83 to 5.58. Carries-over, themselves, with an F of .83, were nowise significant. Perhaps the wisest thing to say is that we should design our

Table XXII - Latin square 6x6x6 showing negative Carry-over

a. Data collected

	Week								
Group	0	1	2	3	4	5	6	Sum	Mean
I	((1))	(1) 51.6	(3) 71.8	(2) 55.9	(5) 69.3	(6) 59.6	(4) 55.1	363.3	60.6
II	((2))	(2) 48.8	(4) 51.4	(3) 52.9	(6) 56.6	(1) 41.6	(5) 54.6	305.9	51.0
III	((3))	(3) 58.0	(5) 58.6	(4) 69.9	(1) 45.4	(2) 72.4	(6) 65.8	370.1	61.7
IV	((4))	(4) 60.9	(6) 45.4	(5) 54.8	(2) 49.6	(3) 55.0	(1) 42.7	308.4	51.4
V	((5))	(5) 61.2	(1) 41.3	(6) 57.2	(3) 59.4	(4) 59.8	(2) 56.4	335.3	55.9
VI	((6))	(6) 61.2	(2) 65.6	(1) 54.8	(4) 65.6	(5) 62.0	(3) 73.1	382.3	63.7
Sum		341.7	334.1	345.5	345.9	350.4	347.7	2065.3	
Mean		57.0	55.7	57.6	57.6	58.4	58.0		57.4

b. Analysis of Treatment effects without allowance for Carry-over

	$(\hat{1})$	$(\hat{2})$	$(\hat{3})$	$(\hat{4})$	$(\hat{5})$	$(\hat{6})$
Contrib.	-11.14	+.75	+4.33	+3.08	+2.71	+.26
Mean	46.23	58.12	61.70	60.45	60.08	57.63

Factors	d.f.	Residual Variability (Squares)
$\hat{\mu}$, Rows & Columns (control)	25	1620.87
Control factors & Treatments	20	659.27

$$F_{5,20} = \frac{(1620.87 - 659.27)/5}{659.27/20} = 5.83^{**}$$

c. Analysis of Treatment effects with allowance for Carry-over

(Treatment estimates as in XXIIIc)

	$\widehat{[1]}$	$\widehat{[2]}$	$\widehat{[3]}$	$\widehat{[4]}$	$\widehat{[5]}$	$\widehat{[6]}$
Contrib.	+3.64	+.97	-1.79	-2.34	+.93	-1.40

Factors	d.f.	Residual Variability (Squares)
$\hat{\mu}$, Rows, Columns & Carries-over (control)	20	1478.55
Control factors & Treatments	15	516.98

$$F_{5,15} = \frac{(1478.55 - 516.98)/5}{516.98/15} = 5.58^{**}$$

Factors	d.f.	Residual Variability (Squares)
$\hat{\mu}$, Rows, Columns & Treatments (control)	20	659.27
Control factors & Carries-over	15	516.98

$$F_{5,15} = \frac{(659.27 - 516.98)/5}{516.98/15} = .83 \text{ N.S.}$$

d. Data rearranged for simple Treatment and Carry-over analysis

	Treatment							
After	(1)	(2)	(3)	(4)	(5)	(6)	Mean	Contrib.
(1)	51.6	72.4	71.8	65.6	54.6	57.2	62.2	+4.8
(2)	54.8	48.8	55.0	51.4	69.3	65.8	57.5	+ .1
(3)	42.7	55.9	58.0	59.8	58.6	56.6	55.3	-2.1
(4)	45.4	56.4	52.9	60.9	62.0	45.4	53.8	-3.6
(5)	41.3	49.6	73.1	69.9	61.2	59.6	59.1	+1.7
(6)	41.6	65.6	59.4	55.1	54.8	61.2	56.3	-1.1
Mean	46.2	58.1	61.7	60.4	60.1	57.6	57.4	
Contrib.	-11.2	+.7	+4.3	+3.0	+2.7	+.2		

experiments so that Carry-over, if it occur, will not bias our calculation of treatment effects. Secondly, we may want to eliminate it from the apparent variability of the participants.

Table XXII^{d} concludes the numerical consideration ; it parallels Table XXIf, by neglecting row effects. This was first done in Table VI. It is a simple but rough way of estimating the effects of direct Treatment and Carry-over. Treatment effects are exactly correct, as previously in Table XXIf, while Carries-over agree reasonably well with those gotten exactly in Table XXIIc.

Analysis with allowance for Carry-over in latin squares without conditioning Period - The previous example of the data of Table VI is actually somewhat unusual, in that there was a conditioning Period when materials or Treatment was tried but no result counts were made. This Period was included only to provide that a given Treatment, such as (1), should be followed by itself as well as all other Treatments. Unfortunately, in practice, the conditioning Period is often, indeed usually, omitted. Practical men are in such a hurry that they do not want to spread their experiment over an extra Period. They want to get results, if possible, from every Period involved in the experiment. The loss is, however, not great and we can still make an analysis for Carry-over. The situation is illustrated by the data of Table XXIIIa, which is a condensation of those of Table XX.

For the first Column or Period of a Design, such as that of Table XXIII, it must be supposed that the model of Equ. (7) still obtains, i.e., there is no term for Carry-over. This is because any background Carry-over that may in truth exist is supposed uniform for all items in the first Column and hence completely confounded with the effect of the first Column.

At least that is the best view taken of the matter; the participants were all under much the same conditions beforehand so whatever the Carry-over is, in fact, it is the same for all Treatments. It is completely confounded with the general effect of the first Column and so for this Column we must simplify our model. Otherwise we proceed as previously for the latin square with a conditioning Period. Equ. (43) is the model obtaining. It does seem a rather strange situation when the model is not the same in all parts of the Design.

The business of analysis again involves setting up least squares equations for the effects, μ , α_i , β_j , γ_k and δ_ℓ appropriate for various levels of the models of Equ. (7) and Equ. (43) and finding the variability variously residual. The handiest thing to do seems to be to set up the full equations, as in Equ. (48), previously, and then cut back to the lesser situations by judiciously dropping rows and columns. Examining Table XXIIIa, it is at once apparent that the grand total 848.2 contains all effects of Rows, Columns, Treatments and Carries-over equally, so that we may write

$$16\hat{\mu} = 848.2 \quad , \tag{59}$$

which is shown as the first line of the grand set of Equ. (66). When it comes to estimating the effect of Rows, it is necessary to recognize that their totals must contain a carry-over effect. This may be done by detailing, as in Equ. (47), the content of, say, Row I and then simplifying on account of the conditions of Equ. (9) and (44) or, once the knack is gotten of simply recognizing that the total, 221.6, for Row I lacks the Carry-over of Treatment (4), and of course contains $\hat{\mu}$ four times. Thus there may be written

$$4\hat{\mu} + 4\hat{\alpha}_1 - \hat{\delta}_3 = 221.6 \tag{60}$$

which is the second line of Equ. (66). Others may be written likewise, as shown in the third and fourth lines. In the fifth line the corresponding statement is not made but rather there is an appeal to the condition of Equ. (9). This is done, again, as in Equ. (48) to avoid the algebraic problem of singularity if Equ. (66) is treated as a set of simultaneous equations. This is to say we write

$$\hat{\alpha}_1 + \hat{\alpha}_2 + \hat{\alpha}_3 + \hat{\alpha}_4 = 0 \quad . \tag{61}$$

For the Columns, which are confounded with nothing, the estimates remain fairly simple; thus for Column 1,

$$4\hat{\mu} + 4\hat{\beta}_1 = 224.2 \tag{62}$$

which becomes a line of Equ. (66). Again the line for Column 4 must be replaced by the condition

$$\hat{\beta}_1 + \hat{\beta}_2 + \hat{\beta}_3 + \hat{\beta}_4 = 0 \quad . \tag{63}$$

The situation for the Treatments is only complicated, as may be seen in Table XXIIIb by, say, Treatment (1) not containing Carry-over [1] so that

$$4\hat{\mu} + 4\hat{\gamma}_1 - \hat{\delta}_1 = 208.0 \tag{64}$$

which becomes the tenth line of Equ. (66). The usual kind of conditioning equation enters. The most intractable case is Carry-over which is now confounded with both Row and Treatment. In particular, consider $\hat{\delta}_1$. Plainly

$$\hat{\alpha}_1 + \hat{\beta}_2 + \hat{\gamma}_2 + \hat{\delta}_1 + \hat{\alpha}_2 + \hat{\beta}_4 + \hat{\gamma}_4 + \hat{\delta}_1 + \hat{\alpha}_4 + \hat{\beta}_3 + \hat{\gamma}_3 + \hat{\delta}_1 + 3\hat{\mu} = 57.2 + 54.0 + 48.6$$

$$3\hat{\delta}_1 + (\hat{\alpha}_1 + \hat{\alpha}_2 + \hat{\alpha}_4) + (\hat{\beta}_2 + \hat{\beta}_3 + \hat{\beta}_4) + (\hat{\gamma}_2 + \hat{\gamma}_3 + \hat{\gamma}_4) + 3\hat{\mu} = 159.8$$

$$3\hat{\mu} - \hat{\alpha}_3 - \hat{\beta}_1 - \hat{\gamma}_1 + 3\hat{\delta}_1 = 159.8 \tag{65}$$

as is shown in the fourteenth line of Equ. (66). The whole situation is as follows:

$$\left.\begin{array}{ccccccccccccccccccr}
\hat{\mu} & \hat{\alpha}_1 & \hat{\alpha}_2 & \hat{\alpha}_3 & \hat{\alpha}_4 & \hat{\beta}_1 & \hat{\beta}_2 & \hat{\beta}_3 & \hat{\beta}_4 & \hat{\gamma}_1 & \hat{\gamma}_2 & \hat{\gamma}_3 & \hat{\gamma}_4 & \hat{\delta}_1 & \hat{\delta}_2 & \hat{\delta}_3 & \hat{\delta}_4 & = & \text{Const.} \\
\hline
+16 & & & & & & & & & & & & & & & & & = & 848.2 \\
+4 & +4 & & & & & & & & & & & & & & -1 & & = & 221.6 \\
+4 & & +4 & & & & & & & & & & & & & & -1 & = & 218.6 \\
+4 & & & +4 & & & & & & & & & & -1 & & & & = & 203.8 \\
 & +1 & +1 & +1 & +1 & & & & & & & & & & & & & = & .0 \\
+4 & & & & & +4 & & & & & & & & & & & & = & 224.2 \\
+4 & & & & & & +4 & & & & & & & & & & & = & 209.6 \\
+4 & & & & & & & +4 & & & & & & & & & & = & 205.8 \\
 & & & & & +1 & +1 & +1 & +1 & & & & & & & & & = & .0 \\
+4 & & & & & & & & & +4 & & & & -1 & & & & = & 208.0 \\
+4 & & & & & & & & & & +4 & & & & -1 & & & = & 215.0 \\
+4 & & & & & & & & & & & +4 & & & & -1 & & = & 206.8 \\
 & & & & & & & & & +1 & +1 & +1 & +1 & & & & & = & .0 \\
+3 & & & -1 & & -1 & & & & -1 & & & & +3 & & & & = & 159.8 \\
+3 & & & & -1 & -1 & & & & & -1 & & & & +3 & & & = & 161.6 \\
+3 & -1 & & & & -1 & & & & & & -1 & & & & +3 & & = & 152.0 \\
 & & & & & & & & & & & & & +1 & +1 & +1 & +1 & = & .0
\end{array}\right\} \quad (66)$$

(The positions left empty contain zeroes, not shown)

In a general way it is first necessary to test for the effect of Treatments when Carry-over is uninvolved in the model. The basic equations are those of (66) but for the moment the elements of Treatment and Carry-over will be neglected. The columns (10th through 17th), involving $\hat{\gamma}_k$ and $\hat{\delta}_\ell$ and the corresponding rows must be dropped. The procedure is along the lines of setting up Equu. (49) from (48). This is a matter of eliminating the control factors. The estimates are:

$$\hat{\mu} = 53.01 \tag{67}$$

(68)

$\hat{\alpha}_1$	$\hat{\alpha}_2$	$\hat{\alpha}_3$	$\hat{\alpha}_4$
+2.39	+1.64	-2.06	-1.96

(69)

$\hat{\beta}_1$	$\hat{\beta}_2$	$\hat{\beta}_3$	$\hat{\beta}_4$
+3.04	-.61	-1.56	-.86

.

It now is necessary to find the residual squares on estimates of $\hat{\mu}$, Rows and Columns as control factors. This requires an operation similar to that of Equ. (28). The procedure is along the lines of setting up Equ. (52) from (48). The residual variability is:

$$\begin{aligned} & 45{,}153.80 - 53.01(848.2) \\ & \quad -\{ + 2.39(221.6) + 1.64(218.6) - 2.06(203.8) - 1.96(204.2)\} \\ & \quad -\{ +3.04(224.2) - .61(209.6) - 1.56(205.8) - .86(208.6)\} \\ & \qquad = 69.38 \end{aligned} \tag{70}$$

or if the thing be carried out with 10-figure accuracy, of the estimates, 71.5025 as in Table XXIIIb. In order to find the further reduction due to Treatments it is necessary to consider their estimates which are shown in Table XXIIIb. Since Treatments are unconfounded with either Rows or Columns, one need only make a further subtraction from the result of Equ. (70) as

$$\begin{aligned} &69.38 \\ &\quad -\{-1.01(208.0) + .74(215.0) - 1.31(206.8) + 1.59(218.4)\} \\ &\qquad = 44.01 \end{aligned} \tag{71}$$

or if the thing be carried out with 10-figure accuracy, of the estimates, so that the results give with those of Table XIXa, 48.2550. These results are summarized in Table XXIIIb; the value F fails significance.

It is, secondly, necessary to test for the effect of Treatments when Carry-over is involved. In order to do this we must find the estimates and residual variability when Treatment is not included and then when it is included. In a general way the columns (10th through 13th) involving $\hat{\gamma}_k$ and corresponding rows of Equ. (66) must be dropped--although this does not affect the numerical solution in the present case. The procedure is along the lines of setting up Equ. (55) from (48). We are making our analysis on the basis of eliminating Carry-over as a control factor, like Rows and Columns. Then we get the estimates:

$$\left.\begin{array}{cccc} \hat{\alpha}_1 & \hat{\alpha}_2 & \hat{\alpha}_3 & \hat{\alpha}_4 \\ \hline +2.24 & +1.29 & -1.90 & -1.63 \\ \hat{\delta}_1[\hat{1}] & \hat{\delta}_2[\hat{2}] & \hat{\delta}_3[\hat{3}] & \hat{\delta}_4[\hat{4}] \\ \hline +.63 & +1.32 & -.59 & -1.37 \end{array}\right\} \tag{72}$$

which differ from those of Equ. (68) because the estimates of $\hat{\delta}_\ell$, or Carry-over, have become involved while the estimates of $\hat{\beta}_j$ remain as there because Carry-over is uninvolved in column estimates. The estimates for $\hat{\delta}_\ell$ or $[\hat{\ell}]$ are as shown in Table XXIc. It is now necessary to find the residual squares on estimates of $\hat{\mu}$, Rows, Columns and Carries-over as control factors.

This requires an operation similar to that of Equ. (28). The residual variability is:

$$
\begin{aligned}
&45{,}153.80 - 53.01(848.2)\\
&-\{+2.24(221.6) + 1.29(218.6) - 1.90(203.8) - 1.63(204.2)\}\\
&-\{+3.04(224.2) - .61(209.6) - 1.56(205.8) - .86(208.6)\}\\
&-\{+.63(159.8) + 1.32(161.6) - .59(152.0) - 1.37(150.6)\}\\
&\quad = 61.15 \qquad (73)
\end{aligned}
$$

or if the thing be carried out with higher accuracy, of the estimates, 59.5000. In order to find the further reduction due to Treatments it is necessary to consider their estimates which are different from those of Table XXIIIb because of their involvement with Carry-over in Equ. (66), they become as shown in Table XXIIIc. The row estimates are affected indirectly to become:

	$\hat{\alpha}_1$	$\hat{\alpha}_2$	$\hat{\alpha}_3$	$\hat{\alpha}_4$
Contrib.	+2.10	+1.42	-1.99	-1.52

. (74)

The residual variability becomes

$$
\begin{aligned}
&45{,}153.80 - 53.01(848.2)\\
&- \{+ 2.10(221.6) + 1.42(218.6) - 1.99(203.8) - 1.52(204.2)\}\\
&- \{+ 3.04(224.2) - .61(209.6) - 1.56(205.8) - .86(208.6)\}\\
&- \{+ .29(159.8) + 1.75(161.6) - 1.17(152.0) - .87(150.6)\}\\
&- \{- .94(208.0) + 1.18(215.0) - 1.60(206.8) + 1.37(218.4)\}\\
&\quad = 30.84 \qquad (75)
\end{aligned}
$$

or if the contributions are calculated with more decimal places the result is 35.0740. These results are summarized in Table XXIIIc. The value of F fails significance even the more dismally with the introduction of Carry-over into the model.

As has been said before, in practice one avoids the effort required to set up the foregoing equations usually by having an electronic computer set them up after it has been given the Design and the data. It then solves the equations and makes the test of significance. The equations are extensive enough to be so tedious that they can hardly be done by hand.

The correlation between Carry-over and direct treatment effect from Table XXIIIc is obviously positive but inconsiderable, in contrast to those gotten previously in connection with Table XXI . The effect of Treatment was, however, inconsiderable, as from the F test and hence could carry-over but little.

In Table XXIIIe there is shown a rough analysis for Treatment and Carry-over analogous to that shown in VIb for the more balanced case. In XXIIIe the Table is not quite complete for reasons as above and it is necessary to make some adjustments in calculating effects as is discussed in the final Chap. IX, on improper fill, where the detailed calculation of this table is shown in Table XLVII. The efficacy of this procedure should be considered. First, it will be noted that the Treatment effects are no longer exactly correct, when compared with Table XXIIIc. This is as one would expect since in this Design they are to some degree confounded with Carry-over, as in Equ. (64), and to a slight degree through Carry-over with block effects. The corresponding Carries-over agree even a little worse. Such discrepancy must arise, of course, from the confounding with Treatments and Blocks of Equ. (64). Still, it might be possible to get some idea of whether there seems to be Carry-over present on such a basis and the man, with limited calculating facilities, might direct his analysis on this basis.

Table XXIII- Results reported by 4 Groups on 4 Treatments over 4 Weeks

a. Data collected

Group	Week 1	Week 2	Week 3	Week 4	Sum
I	(1) 56.8	(2) 57.2	(4) 54.6	(3) 53.0	221.6
II	(2) 57.8	(3) 54.8	(1) 52.0	(4) 54.0	218.6
III	(3) 50.4	(4) 50.6	(2) 50.6	(1) 52.2	203.8
IV	(4) 59.2	(1) 47.0	(3) 48.6	(2) 49.4	204.2
Sum	224.2	209.6	205.8	208.6	848.2
Sum sq.	12,611.88	11,044.24	10,607.48	10,890.20	

b. Analysis for direct Treatment alone

	$\widehat{(1)}$	$\widehat{(2)}$	$\widehat{(3)}$	$\widehat{(4)}$
Contrib.	-1.01	+.74	-1.31	+1.59
Mn.	52.00	53.75	51.70	54.60

Factors	d.f.	Residual Variability (Squares)
$\hat{\mu}$, Rows & Columns (control)	9	71.5025
Control factors plus Treatments	6	48.2550

$$F_{3,6} = \frac{(71.5025 - 48.2550)/3}{48.2550/6} = .96 \text{ N.S.}$$

c. Direct Treatment and Carry-over estimates from simultaneous equations

	$\widehat{(1)}$	$\widehat{(2)}$	$\widehat{(3)}$	$\widehat{(4)}$
Contrib.	-.94	+1.18	-1.60	+1.37
Mn.	52.92	53.13	52.85	53.15

$\widehat{[1]}$	$\widehat{[2]}$	$\widehat{[3]}$	$\widehat{[4]}$
+.29	+1.75	-1.17	-.87

d. Test of significance of treatment effects

Factors	d.f.	Residual Variability (Squares)
$\hat{\mu}$, Rows, Columns, Carries-over (control)	6	59.5000
Control factors plus Treatments	3	35.0740

$$F_{3,3} = \frac{(59.5000 - 35.0740)/3}{35.0740/3} = .70 \text{ N.S.}$$

e. Data rearranged by Treatment and Treatment of Preceding Week

	Treatment							
After	(1)	(2)	(3)	(4)	Sum	Mn	Adj. Mn.	Contrib.
(1)		57.2	48.6	54.0	159.8	53.3	53.0	+1.0
(2)	52.2		54.8	54.6	161.6	53.9	54.3	+2.3
(3)	52.0	49.4		50.6	152.0	50.6	50.1	-1.9
(4)	47.0	50.6	53.0		150.6	50.2	50.6	-1.4
Bk. gd.	56.8	57.8	50.4	59.2	224.2	56.0		
Sum	208.0	215.0	206.8	218.4	848.2			
Mn.	52.0	53.8	51.7	54.6				
Adj. Mn.	52.2	54.3	51.2	54.2				
Contrib.	-.8	+1.3	-1.8	+1.2				

In the actual conduct of the experiment just considered one would not, of course, work with the data in the condensed form of Table XXIII rather in the original form of Table XX , i.e., with 20 Rows. The necessary equations are much the same but more numerous. The same program for an electronic computer may be used. The analysis is much the same except that the number of degrees of freedom are greater than in Table XXIIId. The test of significance of treatment effects becomes

Factors	d.f.	Residual Variability (Squares)
$\hat{\mu}$, Rows, Columns, Carries-over (control)	54	3585.5000
Control factors plus Treatments	51	3463.3700

$$F_{3,51} = \frac{(3585.5000 - 3463.3700)/3}{3463.3700/51} = .60 \text{ N.S.}$$

Explicit solution for simple cases of Carry-over - There is a strong temptation for people with ability in handling equations to take the least squares equations of the style of Equ. (48) or (66) and write out explicit solutions for the estimates of direct treatment effects, $\hat{\gamma}_k$, and Carry-over, $\hat{\delta}_\ell$, in terms of various sums of the observations. This is possible for cases when, t , the dimension is small. Such temptation should be, in general, resisted. It may, nonetheless, be illuminating here to consider some of the smaller cases.

The situation for the smallest of latin squares, 2x2x2, is of particular practical importance and peculiar theoretical difficulty. It is discussed at some length in Chap. VIII, which is devoted to paired comparisons, i.e., Designs with 2 Columns, of which this is a case.

The next smallest latin square, 3x3x3, has t odd and so, as in Chap. IV, is not recommended because the Carry-over is ill-behaved. Paired latin squares are recommended there. One writes the satisfactory paired latin square Design, 2(3x3x3) as follows:

	Period		
Group	1	2	3
I	$^{(1)}y_{111}$	$^{(2)}y_{1221}$	$^{(3)}y_{1332}$
II	$^{(2)}y_{212}$	$^{(3)}y_{2232}$	$^{(1)}y_{2313}$
III	$^{(3)}y_{313}$	$^{(1)}y_{3213}$	$^{(2)}y_{3321}$
IV	$^{(1)}y_{411}$	$^{(3)}y_{4231}$	$^{(2)}y_{4323}$
V	$^{(2)}y_{512}$	$^{(1)}y_{5212}$	$^{(3)}y_{5331}$
VI	$^{(3)}y_{613}$	$^{(2)}y_{6223}$	$^{(1)}y_{6312}$

It is possible to solve for row, column, treatment and carry-over effects since there are 12 parameters:

$\hat{\mu}$	1		
Groups	5	Treatments	2
Periods	2	Carries-over	2

to be discovered from 18 observations. The situation is, indeed, over-determined with 6 degrees of freedom, which are available for estimating the extraneous or residual variability. If the Design is repeated twice there would be 12 such degrees of freedom. For the Design, as it stands above, without a conditioning Period, one may set up the algebraic least squares equations involved and then solve for the various kinds of effect explicitly in terms of sums of observations, but the results would be nigh intolerable.

The previous example was the 2(3x3x3) unconditioned. The conditioned Design, i.e., with a Period 0 where Treatment (1) preceded (1) in Period 1, Treatment (2) preceded (2) and Treatment (3) preceded (3), would be as follows:

	Period			
Group	0	1	2	3
I	((1))	(1) y_{1111}	(2) y_{1221}	(3) y_{1332}
II	((2))	(2) y_{2122}	(3) y_{2232}	(1) y_{2313}
III	((3))	(3) y_{3133}	(1) y_{3213}	(2) y_{3321}
IV	((1))	(1) y_{4111}	(3) y_{4231}	(2) y_{4323}
V	((2))	(2) y_{5122}	(1) y_{5212}	(3) y_{5331}
VI	((3))	(3) y_{6133}	(2) y_{6223}	(1) y_{6312}

It is, again, possible to solve for row, column, treatment and carry-over effects with 6 degrees of freedom, left over for the estimation of extraneous variability. The least squares equations, here, are peculiarly simple and could be handled on a desk calculator. Even here, however, it is most simple to use the electronic program as in the Appendix.

For the 4x4x4 Design,

	Period			
Group	1	2	3	4
I	(1) y_{111}	(2) y_{1221}	(4) y_{1342}	(3) y_{1434}
II	(2) y_{212}	(3) y_{2232}	(1) y_{2313}	(4) y_{2441}
III	(3) y_{313}	(4) y_{3243}	(2) y_{3324}	(1) y_{3412}
IV	(4) y_{414}	(1) y_{4214}	(3) y_{4331}	(2) y_{4423}

it is similarly possible to solve since there are 13 parameters to be discovered from 16 observations. For the Design with previous conditioning Period, it is again possible to get $\hat{\mu}$ and direct treatment estimates from simple averages. Otherwise explicit algebraic solution is intolerable. The least squares equations should be set up and solved, numerically, as previously. For all latin square Designs, $t \times t \times t$, $t > 2$ and even, the same can be said. If we attempt to write out explicit solutions for latin squares, we shall encounter difficulty in defining sums of various kinds.

Latin squares with missing Rows - Under the practical conditions of experimentation it is very common, indeed almost usual, for there to be some data missing from any latin square Design of any type. This is to say that the Design, as originally laid out, is in some measure not completed. The misses may be conceived as of two types. First that with the odd cell or cells missed. This is the type usually discussed in the literature although it is not often found in practice. Second that where a whole line or whole lines of Design is or are missing. This is very common in the actual execution of experiments, although it does not seem to be discussed extensively in the literature.

Let us consider common ways in which the problem of Designs incomplete by Rows may arise. Perhaps one has 15 patients on whom one wants to try 6 Treatments. It seems wilful and extravagant to lay out only two 6x6x6 latin squares when the remaining 3 men will give more information and one is compelled to add the first 3 Rows of a third latin square. The way things finally go may be imagined. We may plan to use a balanced Change-over 6x6x6, as in Table VIII, and use it 2.5 times over to produce a Design with 15 Rows and 6 Columns. Since each line is associated with a man, and men tend to disappoint us, we may find ourselves with certain of the lines of the Design done 3 times, a number done twice, and one or two done once. Accordingly, it becomes a matter of dealing with a Design where balance is much disturbed. Similarly, one may plan to test 7 Treatments on 7 Machines in 7 Periods. One of the Machines assigned is, however, withdrawn just after the experiment starts and no replacement is possible. Now it is idle to tell an engineer interested in the Treatments on the Machines that since Treatments are no longer orthogonal to Periods the work must be abandoned. He will reply that he can get about 6/7 of the information anyhow and proceed to get it. The biometrician must do the best he can with the incomplete Design.

By way of illustrating the loss of Rows of the Design, a far more modest example is given in Table XXIV, where the last Row of Table VI has been deliberately lost. There is no great problem. One simply applies the appropriate least squares equations, which are probably on an electronic computer, as in the program of the Appendix. The procedure of setting up the equations may, however, sometimes be of practical use and certainly is of interest in justifying the program alluded to. In Table XXIVa there is shown this reduced Design and resultant data. There has been added to each cell the Carry-over, as well as the usual Treatment and result. This has been done because it is a little difficult to find one's way about such a disturbed situation.

Examining Table XXIVa, it is at once apparent that the grand total 1661.1 contains all effects of Rows, Columns and Treatments equally, so that in view of Equ. (9) they may be ignored. The grand total does not, however, contain the Carries-over equally; [2], [3], [5] and [6] occur each five times whereas [1] occurs six times and [4] occurs four times. More economically we may say, in view of Equ. (44), that [1] occurs once too often and [4] once too infrequently so that we may write

$$30\hat{\mu} + \hat{\delta}_1 - \hat{\delta}_4 = 1661.1 \quad , \qquad (76)$$

which is shown as the first line of the grand set of Equ. (81). When it comes to estimating the effect of Rows, it is necessary to recognize that each of their totals must contain a carry-over effect, as previously in Equ. (48); this effect is not due to the line of the Design being missing. Thus, again, we may recognize that the total, 317.0, for Row I, contains the Carry-over of Treatment (1) once too often but lacks that of (4), and of course contains μ six times. Thus there may be written

$$6\hat{\mu} + 6\hat{\alpha}_1 + \hat{\delta}_1 - \hat{\delta}_6 = 317.0 \tag{77}$$

which is the second line of Equ. (81). Others may be written likewise, as shown in the third through sixth lines. In the seventh line the corresponding statement is not made but rather there is an appeal to the condition of Equ. (9). This is done, again, as in Equ. (48) to avoid the algebraic problem of singularity. Even the Columns, now, are confounded with the effect of Treatments and Carries-over. Thus Column 1 lacks Treatment (4) and Carry-over [4] so that we must say

$$5\hat{\mu} + 5\hat{\beta}_1 - \hat{\gamma}_4 - \hat{\delta}_4 = 269.3 \tag{78}$$

which becomes a line of Equ. (81). Again the line for Column 6 must be replaced by the condition

$$\hat{\beta}_1 + \hat{\beta}_2 + \ldots + \hat{\beta}_6 = 0 \quad . \tag{79}$$

The situation for the Treatments is complicated by, say, the total for Treatment (1) not containing Column 6 nor Carry-over [3] so that

$$5\hat{\mu} - \hat{\beta}_6 + 5\hat{\gamma}_1 - \hat{\delta}_3 = 264.8 \tag{80}$$

which becomes the fourteenth line of Equ. (81). The usual kind of conditioning equation enters. The most intractable situation exists for Carry-over which has been variously disturbed by the missing Row of the Design.

Thus, as in the twentieth line of Equ. (81), all six possible values of $\hat{\delta}_1$ are present and the appropriate line of those equations is very simple. At the other extreme only four of the possible values of $\hat{\delta}_4$ are present and it is heavily confounded with every other type of effect. The whole situation is as follows:

$\hat{\mu}$	$\hat{\alpha}_1$	$\hat{\alpha}_2$	$\hat{\alpha}_3$	$\hat{\alpha}_4$	$\hat{\alpha}_5$	$\hat{\beta}_1$	$\hat{\beta}_2$	$\hat{\beta}_3$	$\hat{\beta}_4$	$\hat{\beta}_5$	$\hat{\beta}_6$	$\hat{\gamma}_1$	$\hat{\gamma}_2$	$\hat{\gamma}_3$	$\hat{\gamma}_4$	$\hat{\gamma}_5$	$\hat{\gamma}_6$	$\hat{\delta}_1$	$\hat{\delta}_2$	$\hat{\delta}_3$	$\hat{\delta}_4$	$\hat{\delta}_5$	$\hat{\delta}_6$	=	Sum
+30																		+1			-1			=	1661.1
+6	+6																	+1			-1			=	317.0
+6		+6																	+1			-1		=	336.5
+6			+6																	+1			-1	=	336.8
+6				+6															-1			+1		=	344.3
	+1	+1	+1	+1	+1																			=	.0
+5						+5									-1						-1			=	269.3
+5							+5										-1				-1			=	264.1
+5								+5								-1							-1	=	258.1
+5									+5				-1									-1		=	296.0
+5										+5				-1					-1					=	288.8
						+1	+1	+1	+1	+1	+1													=	.0
+5											-1	+5								-1				=	264.8
+5									-1				+5									-1		=	283.0
+5										-1				+5					-1					=	253.2
+5						-1									+5						-1			=	300.3
+5								-1								+5							-1	=	262.8
												+1	+1	+1	+1	+1	+1							=	.0
+6	+1																	+6						=	308.6
+5		+1		-1						-1				-1					+5					=	287.6
+5			+1		-1						-1	-1								+5				=	270.6
+4	-1					-1	-1								-1		-1				+4			=	219.6
+5		-1		+1					-1				-1									+5		=	276.9
																		+1	+1	+1	+1	+1	+1	=	.0

(81)

(The positions left empty contain zeroes, not shown)

Let us first make the analysis for treatment effects, without allowance for Carry-over, towards the results of Table XXIVb. The basic equations are Equ. (81), but for the moment the elements of Treatment and Carry-over will be neglected. The columns (14 through 25) involving $\hat{\gamma}_k$ and $\hat{\delta}_\ell$ and the corresponding rows must be dropped. The procedure is, again, along the lines of setting up Equ. (49) from (48). This is a matter of eliminating the control factors. The estimates are:

$$30\hat{\mu} = 1661.1$$
$$\hat{\mu} = 55.37 \qquad (82)$$

and, for the moment,

$\hat{\alpha}_1$	$\hat{\alpha}_2$	$\hat{\alpha}_3$	$\hat{\alpha}_4$	$\hat{\alpha}_5$
-2.54	+.71	+.76	+2.01	-.95

(83)

$\hat{\beta}_1$	$\hat{\beta}_2$	$\hat{\beta}_3$	$\hat{\beta}_4$	$\hat{\beta}_5$	$\hat{\beta}_6$
-1.51	-2.55	-3.75	+3.83	+2.39	+1.59

(84)

It now is necessary to find the residual squares on estimates of $\hat{\mu}$, Rows and Columns as control factors. This residual variability is:

$$\begin{aligned} &93{,}312.21 - 55.37(1661.1) \\ &\quad - \{-2.54(317.0) + .71(336.5) + .76(336.8) + 2.01(344.3) - .95(326.5)\} \\ &\quad - \{-1.51(269.3) - 2.55(264.1) - 3.75(258.1) + 3.83(296.0) + 2.39(288.8) \\ &\qquad + 1.59(284.8)\} \\ &= 1036.76 \end{aligned} \qquad (85)$$

or if the thing be carried out with more decimal places in the estimates, 1033.19 as in Table XXIVb. In order to find the further reduction due to Treatments it is necessary to consider those estimates. The estimate of

μ remains as in Equ. (82) because the total contains all Treatments equally, like everything else. The estimates of $\hat{\alpha}_i$ remain as in Equ. (83). The estimates for the Columns are, however, changed since they now contain various sets of Treatments. In a general way, one simply ignores the 20th to 27th columns and corresponding rows of Equ. (81). Thus for the first Column of the Design,

$$5\hat{\mu} + 5\hat{\beta}_1 - \hat{\gamma}_4 = 269.3 \tag{86}$$

and one gets the estimates

$$\begin{array}{cccccc} \hat{\beta}_1 & \hat{\beta}_2 & \hat{\beta}_3 & \hat{\beta}_4 & \hat{\beta}_5 & \hat{\beta}_6 \\ \hline -.60 & -1.82 & -4.49 & +4.25 & +1.50 & +1.15 \end{array} . \tag{87}$$

From the same reduced matrix, the equation for Treatment (1) is

$$5\hat{\mu} - \hat{\beta}_6 + 5\hat{\gamma}_1 = 264.8 \tag{88}$$

and thus one gets the estimates for Treatment as in Table XXIVb. Then the residual variability is

$$\begin{aligned} &93{,}312.21 - 55.37(1661.1) \\ &\quad -\{-2.54(317.0) + .71(336.5) + .76(336.8) + 2.01(344.3) - .95(326.5)\} \\ &\quad -\{-.60(269.3) - 1.82(264.1) - 4.49(258.1) + 4.25(296.0) + 1.50(288.8) \\ &\qquad + 1.15(284.8)\} \\ &\quad -\{-2.18(264.8) + 2.08(283.0) - 4.43(253.2) + 4.57(300.3) - 3.71(262.8) \\ &\qquad + 3.67(297.0)\} \\ &\quad = 670.85 \end{aligned} \tag{89}$$

or if the thing be carried out with more decimal places on the estimates, 664.37 as in Table XXIVb.

It is, secondly, necessary to test for the effect of Treatments when Carry-over is involved. In order to do this we must find the estimates and residual variability when Treatment is not included and then when it is included. In a general way the columns (14 through 19) involving $\hat{\gamma}_k$ and corresponding rows of Equ. (81) must be dropped. The estimate for $\hat{\mu}$ is no longer as in Equ. (82) because, as can be seen in Equ. (81), there is confounding with $\hat{\delta}_\ell$. Now

$$\hat{\mu} = 55.41 \tag{90}$$

Other estimates become

$$\left.\begin{array}{cccccc}
 & \hat{\alpha}_1 & \hat{\alpha}_2 & \hat{\alpha}_3 & \hat{\alpha}_4 & \hat{\alpha}_5 \\
\cline{2-6}
 & -2.38 & +.20 & +1.74 & -2.45 & -2.01 \\
\hat{\beta}_1 & \hat{\beta}_2 & \hat{\beta}_3 & \hat{\beta}_4 & \hat{\beta}_5 & \hat{\beta}_6 \\
\hline
-2.03 & -3.07 & -2.93 & +3.85 & +2.98 & +1.19 \\
\hat{\delta}_1 & \hat{\delta}_2 & \hat{\delta}_3 & \hat{\delta}_4 & \hat{\delta}_5 & \hat{\delta}_6 \\
\hline
-3.58 & +3.16 & -1.80 & -2.38 & +.29 & +4.32
\end{array}\right\} \tag{91}$$

The residual variability is still quite simple as

$$\begin{aligned}
&93{,}312.21 - 55.41(1661.1) \\
&\quad - \{ - 2.38(317.0) + .20(336.5) + \ldots\ldots\ldots\ldots - 2.01(326.5)\} \\
&\quad - \{ - 2.03(269.3) - 3.07(264.1) - \ldots\ldots\ldots\ldots + 1.19(284.8)\} \\
&\quad - \{ - 3.58(308.6) + 3.16(287.6) - \ldots\ldots\ldots\ldots + 4.32(297.8)\} \\
&\qquad = 797.98
\end{aligned} \tag{92}$$

or if the thing be carried out with more decimal places in the estimates, the 797.76 as in Table XXIVc. In order to find the further reduction due to Treatments it is necessary to solve the full set of Equ. (81) and get

$$\left.\begin{array}{rrrrrr}
 & & \hat{\mu} = 55.50 & & & \\
 & \hat{\alpha}_1 & \hat{\alpha}_2 & \hat{\alpha}_3 & \hat{\alpha}_4 & \hat{\alpha}_5 \\
\cline{2-6}
 & -2.01 & +.40 & +1.65 & +2.06 & -2.11 \\
\hat{\beta}_1 & \hat{\beta}_2 & \hat{\beta}_3 & \hat{\beta}_4 & \hat{\beta}_5 & \hat{\beta}_6 \\
\hline
-.70 & -1.92 & -3.80 & +4.29 & +1.81 & +.32
\end{array}\right\} \qquad (93)$$

together with the estimates of $\hat{\gamma}_k$ or $(\hat{k})$ and $\hat{\delta}_\ell$ or $[\hat{\ell}]$ of Table XXIVc. The residual variability becomes

$$\begin{aligned}
&93{,}312.21 - 55.50(1661.1) \\
&\quad - \{-2.01(317.0) + .40(336.5) + \dots\dots\dots\dots - 2.11(326.5)\} \\
&\quad - \{-.70(269.3) - 1.92(264.1) - \dots\dots\dots\dots + .32(284.8)\} \\
&\quad - \{-3.01(264.8) + 2.12(283.0) - \dots\dots\dots\dots + 3.56(297.0)\} \\
&\quad - \{-3.74(308.6) + 1.88(287.6) - \dots\dots\dots\dots + 3.45(297.8)\} \\
&\qquad = 495.80
\end{aligned} \qquad (94)$$

or if the thing be carried out with more decimal places in the estimates the 489.73 of Table XXIVc.

As has been said before, in practice one avoids the effort required to set up the foregoing equations usually by having an electronic computer do so, after it has been given the Design and the data, in the electronic program of the Appendix. That then solves the equations and makes the test of significance. The equations are extensive enough to be so tedious that they can hardly be done by hand. The confounding is horrific.

In Table XXIVb there is shown below the contributions for Treatments the Adjusted Mean--not the Mean as previously. The new quantity is found by adding $\hat{\gamma}_k$ to $\hat{\mu}$. It is essentially an estimate of what mean effect of Treatment would have been if some sort of confounding had not occurred.

Table XXIV- Satisfaction of 6 Groups over 6 Weeks with 6 Treatments when 1 Row is missing

a. Data from Table VI with last Row missing

Group	0	Week 1	2	3	4	5	6	Sum	Mean
I	((1))	(1)46.4[1]	(3)45.8[1]	(2)40.8[3]	(5)62.4[2]	(6)59.9[5]	(4)61.7[6]	317.0	52.8
II	((2))	(2)60.9[2]	(4)59.2[2]	(3)44.9[4]	(6)64.2[3]	(1)55.3[6]	(5)52.0[1]	336.5	56.1
III	((3))	(3)50.0[3]	(5)50.0[3]	(4)64.2[5]	(1)60.9[4]	(2)58.4[1]	(6)53.3[2]	336.8	56.1
IV	((5))	(5)48.8[5]	(1)50.4[5]	(6)56.4[1]	(3)58.9[6]	(4)65.6[3]	(2)64.2[4]	344.3	57.4
V	((6))	(6)63.2[6]	(2)58.7[6]	(1)51.8[2]	(4)49.6[1]	(5)49.6[4]	(3)53.6[5]	326.5	54.4
Sum		269.3	264.1	258.1	296.0	288.8	284.8	1661.1	
Mean		53.9	52.8	51.6	59.2	57.8	57.0		55.4

b. Analysis for direct Treatment alone

	$(\hat{1})$	$(\hat{2})$	$(\hat{3})$	$(\hat{4})$	$(\hat{5})$	$(\hat{6})$
Contrib	-2.18	+2.08	-4.43	+4.57	-3.71	+3.67
Adj. Mn.	53.19	57.45	50.94	59.94	51.66	59.04

Factors	d.f.	Residual Variability (Squares)
$\hat{\mu}$, Rows & Columns (control)	20	1033.19
Control factors plus Treatments	15	664.37

$$F_{5,15} = \frac{(1033.19 - 664.37)/5}{664.37/15} = 1.67 \text{ N.S.}$$

c. Analysis for direct Treatment, with allowance for Carry-over

	$\widehat{(1)}$	$\widehat{(2)}$	$\widehat{(3)}$	$\widehat{(4)}$	$\widehat{(5)}$	$\widehat{(6)}$
Contrib	-3.01	+2.12	-4.12	+4.47	-3.01	+3.56
Adj. Mn.	52.49	57.62	51.38	59.97	52.49	59.06

$\widehat{[1]}$	$\widehat{[2]}$	$\widehat{[3]}$	$\widehat{[4]}$	$\widehat{[5]}$	$\widehat{[6]}$
-3.74	+1.88	-2.67	+.25	+.83	+3.45

Factors	d.f.	Residual Variability (Squares)
$\hat{\mu}$, Rows, Columns & Carries-over (control)	15	797.76
Control factors plus Treatments	10	489.73

$$F_{5,10} = \frac{(797.76 - 489.73)/5}{489.73/10} = 1.26 \text{ N.S.}$$

d. Data arranged by Treatment and by Treatment of the preceding Week

	Treatment								
After	(1)	(2)	(3)	(4)	(5)	(6)	Sum	Adj. Mean	Contr.
(1)	46.4	58.4	45.8	49.6	52.0	56.4	308.6	51.4	-4.2
(2)	51.8	60.9		59.2	62.4	53.3	287.6	56.6	+1.0
(3)		40.8	50.0	65.6	50.0	64.2	270.6	53.5	-2.1
(4)	60.9	64.2	44.9		49.6		219.6	57.1	+1.5
(5)	50.4		53.6	64.2	48.8	59.9	276.9	55.6	.0
(6)	55.3	58.7	58.9	61.7		63.2	297.8	59.1	+3.5
Sum	264.8	283.0	253.2	300.3	262.8	297.0	1661.1		
Mean	53.96	56.60	50.64	60.06	52.56	59.40			
Adj.Mn.	52.5	56.6	50.8	60.4	53.3	59.7			
Contr.	-3.1	+1.0	-4.8	+4.8	-2.3	+4.1		55.6	

In the present case it argues as to the mean effect of Treatment, if that had occurred equally in all Columns. There is a similar Adjusted Mean in Table XXIVc which is essentially an estimate of what mean effect of Treatment would have been if each had occurred equally in all Columns and with the same Carry-over.

It may be of interest to compare the estimates of Table XXIVc with those gotten on the full data as in Table XXI a and b. Perhaps one may say that the estimates for Treatment and Carry-over are substantially the same but the value of F drops from 2.12 to 1.26 and this with decreased degrees of freedom, which makes things worse.

In Table XXIVd there is again shown a rough analysis for Treatment and Carry-over analogous to that shown in VIb for the more balanced case. This table is, of course, far from complete and it is necessary to make some adjustments in calculating effects as is discussed in the final Chap. IX, on improper fill.

In Table XXIV the number of degrees of freedom consumed by estimates of the Rows is, of course, less than that in the full Design. The number left after the control and experimental factors have been handled is the number ascribed to the residual variability. This number is also reduced from that for the full Design.

The problem of breaking up variability among Treatment effects may arise in a situation where there are missing data. A slightly approximate but satisfactory procedure is that previously discussed at length in connection with single, complete latin squares. It may be illustrated on the case of Table XXIVb, where a Row is missing. The value of K would be difficult to calculate exactly. Accordingly, the empirical value has to be calculated in all earnestness, along the lines of Equ. (36) as

$$K = \frac{1033.19 - 664.37}{(-2.18)^2 + (+2.08)^2 + (-4.43)^2 + (+4.57)^2 + (-3.71)^2 + (+3.67)^2}$$

$$= 4.801 \quad . \qquad (95)$$

To cover again some of the ground of the earlier and more complete example, consider:

a. The comparison of Treatments $(\hat{1})$ and $(\hat{3})$ with all other Treatments as shown in Equ. (31) and (37). There must be calculated

$$4.801\{\frac{(-2.18 - 4.43)^2}{2} + \frac{(+2.08 + 4.57 - 3.71 + 3.67)^2}{4}\}$$

$$= 157.3239 \quad . \qquad (96)$$

Now this value is to be compared with the final residual variability of Table XXIVb to give

$$F_{1,15} = \frac{157.3239}{664.37/15} = 3.55 \qquad (97)$$

which on reference to Table L is no longer significant at the 5% level.

b. The comparison of $(\hat{2})$ and $(\hat{3})$ with $(\hat{4})$, $(\hat{5})$ and $(\hat{6})$, as shown in Equ. (33) and (39). Now there must be calculated

$$4.801\{\frac{(+2.08 - 4.43)^2}{2} + \frac{(+4.57 - 3.71 + 3.67)^2}{3}$$

$$- \frac{(+2.08 - 4.43 + 4.57 - 3.71 + 3.67)}{5}\}$$

$$= 41.5339 \quad . \qquad (98)$$

Now this value is to be compared with the final residual variability of Table XXIVb, to give

$$F_{1,15} = \frac{41.5339}{664.37/15} = .94 \tag{99}$$

which on reference to Table L proves not significant. Since in the present case, each Treatment is represented the same number (five) times the tests of significance in connection with Equ. (97) and (99) are exact. If,in fact, the various Treatments had had various numbers of observations (as can happen) the test of significance of the F , with a single degree of freedom in the numerator, would be disturbed.

Latin squares with missing cells - Let us, secondly, consider the situation, more trivial but so dear to the literature, where a Design is incomplete by occasional values or cells. This situation may arise in various ways. Perhaps one machine of a series breaks down after supplying 2 or 3 observations; some man fails, in part, to complete his assignment of Changes-over. This situation is illustrated by the deliberate loss of data from Table VI, a single latin square; this compact Design is convenient for our purpose. Two cells were dropped from Table VI to produce Table XXV. Against each observation there is shown, as previously in Table XXIV, not only the Treatment proper but the Carry-over. In the case of the two missing observations (NR) it is supposed they were administered and that there is Carry-over in the following observation. In practice, there is often a missed administration of Treatment besides no observation, so that there is no Carry-over. This alternative can be easily enough picked up in the appropriate least squares equations. The procedure of analysis is just as for the previous case with a missing Row. It will be summarized a little more briefly, mainly by giving the equations in matrix form.

It is perhaps best, again, to set up the total least squares equations for Table XXV , just as Equ. (48) was set up for Table XXI . Many of the rows will be indeed repeated. The grand total lacks the Rows, Columns, Treatments and Carries-over properly involved in the missing (N.R.) cells and this shows up in the first line of Equ.(100). On the other hand, the second line for $\hat{\alpha}_1$ is exactly as in Equ. (48). The third line for $\hat{\alpha}_2$ is disturbed by the missing cell. Similarly the lines for some of the Columns of the Design are disturbed as are those for some of the Treatments and Carries-over. The whole situation is as follows:

$\hat{\mu}$	$\hat{\alpha}_1$	$\hat{\alpha}_2$	$\hat{\alpha}_3$	$\hat{\alpha}_4$	$\hat{\alpha}_5$	$\hat{\alpha}_6$	$\hat{\beta}_1$	$\hat{\beta}_2$	$\hat{\beta}_3$	$\hat{\beta}_4$	$\hat{\beta}_5$	$\hat{\beta}_6$	$\hat{\gamma}_1$	$\hat{\gamma}_2$	$\hat{\gamma}_3$	$\hat{\gamma}_4$	$\hat{\gamma}_5$	$\hat{\gamma}_6$	$\hat{\delta}_1$	$\hat{\delta}_2$	$\hat{\delta}_3$	$\hat{\delta}_4$	$\hat{\delta}_5$	$\hat{\delta}_6$	=	Sum
34		-1				-1		-1			-1					-1	-1			-1		-1			=	1923.0
6	+6																		+1			-1			=	317.0
5		+5						-1								-1							-1		=	277.3
6			+6																		+1			-1	=	336.8
6				+6															-1			+1			=	370.7
6					+6															-1			+1		=	344.3
	+1	+1	+1	+1	+1	+1																			=	.0
6							+6																		=	333.0
5		-1						+5								-1				-1					=	276.9
6									+6																=	329.8
6										+6															=	353.3
5						-1					+5						-1					-1			=	292.5
							+1	+1	+1	+1	+1	+1													=	.0
6													+6												=	317.5
6														+6											=	340.3
6															+6										=	306.5
5		-1						-1								+5				-1					=	304.8
5						-1					-1						+5					-1			=	284.9
													+1	+1	+1	+1	+1	+1							=	.0
6	+1			-1															+6						=	308.6
5					-1			-1								-1				+5					=	281.7
6			+1			-1															+6				=	323.3
5	-1			+1		-1					-1						-1					+5			=	305.7
6		-1			+1																		+6		=	334.2
																			+1	+1	+1	+1	+1	+1	=	.0

(100)

(The positions left empty contain zeroes, not shown)

First there must be considered the model with only the control factors $\hat{\mu}$, Rows and Columns. The elements of Treatment and Carry-over will be neglected, i.e., the columns (14th through 25th) involving $\hat{\gamma}_i$ and $\hat{\delta}_\ell$ of Equ. (100) and the corresponding rows must be dropped. The resultant estimates are:

$$\left.\begin{array}{cccccc}
\multicolumn{6}{c}{\hat{\mu} = 56.51} \\
\hat{\alpha}_1 & \hat{\alpha}_2 & \hat{\alpha}_3 & \hat{\alpha}_4 & \hat{\alpha}_5 & \hat{\alpha}_6 \\
\hline
-3.68 & -1.33 & -.38 & +5.27 & +.87 & -.76 \\
\hat{\beta}_1 & \hat{\beta}_2 & \hat{\beta}_3 & \hat{\beta}_4 & \hat{\beta}_5 & \hat{\beta}_6 \\
\hline
-1.01 & -1.40 & -1.54 & +2.37 & +1.84 & -.26
\end{array}\right\} . \qquad (101)$$

It now is necessary to find the residual squares on estimates of $\hat{\mu}$, Rows and Columns as control factors, which is:

$$\begin{aligned}
&110{,}631.46 - 56.51(1923.0) \\
&\quad - \{ - 3.68(317.0) - 1.33(277.3) - \dots - .76(276.9)\} \\
&\quad - \{ - 1.01(333.0) - 1.40(276.9) - \dots - .26(337.5)\} \\
&\qquad = 1523.52 \qquad (102)
\end{aligned}$$

if the thing be carried out with 4-figure accuracy, of the estimates. In order to find the further reduction due to Treatments it is necessary to consider Equ. (100) less the 20th through 25th columns and the corresponding rows. The resultant estimates are:

$$\left.\begin{array}{cccccc} \multicolumn{6}{c}{\hat{\mu} = 56.68} \\ \hat{\alpha}_1 & \hat{\alpha}_2 & \hat{\alpha}_3 & \hat{\alpha}_4 & \hat{\alpha}_5 & \hat{\alpha}_6 \\ \hline -3.85 & -.53 & -.55 & +5.10 & +.70 & -.86 \\ \hat{\beta}_1 & \hat{\beta}_2 & \hat{\beta}_3 & \hat{\beta}_4 & \hat{\beta}_5 & \hat{\beta}_6 \\ \hline -1.18 & -.60 & -1.72 & +2.20 & +1.74 & -.43 \end{array}\right\} . \qquad (103)$$

and estimates of $\hat{\gamma}_k$ or $(\hat{k})$, as shown in Table XXVb. The residual is

$$110{,}631.46 - 56.68(1923.0)$$
$$- \{ - 3.85(317.0) - .53(277.3) - \ldots - .86(276.9)\}$$
$$- \{ - 1.18(333.0) - .60(276.9) - \ldots - .43(337.5)\}$$
$$- \{ - 3.77(317.5) + .03(340.3) - \ldots + 4.82(369.0)\}$$
$$= 1034.7991 \qquad (104)$$

if the thing be carried out with 4-figure accuracy, of the estimates. These results are summarized in Table XXVb; the value F fails significance, as it did in the full, original table of data.

Next there must be considered the model with only the control factors $\hat{\mu}$, Rows, Columns and Carries-over. The columns (14th through 19th) involving $\hat{\gamma}_k$ and corresponding rows of Equ. (100) must be dropped. The resultant estimates are:

$$\left.\begin{array}{cccccc} \multicolumn{6}{c}{\hat{\mu} = 56.57} \\ \hat{\alpha}_1 & \hat{\alpha}_2 & \hat{\alpha}_3 & \hat{\alpha}_4 & \hat{\alpha}_5 & \hat{\alpha}_6 \\ \hline -2.49 & -1.69 & +1.01 & +3.98 & +.98 & -1.79 \\ \hat{\beta}_1 & \hat{\beta}_2 & \hat{\beta}_3 & \hat{\beta}_4 & \hat{\beta}_5 & \hat{\beta}_6 \\ \hline -1.07 & -1.59 & -1.60 & +2.32 & +2.25 & -.32 \\ \hat{\delta}_1 & \hat{\delta}_2 & \hat{\delta}_3 & \hat{\delta}_4 & \hat{\delta}_5 & \hat{\delta}_6 \\ \hline -4.05 & -.35 & -3.15 & +3.37 & -1.31 & +5.48 \end{array}\right\} . \qquad (105)$$

It now is necessary to find the residual squares on estimates of $\hat{\mu}$, Rows, Columns and Carries-over as control factors, which is:

$$\begin{aligned}
&110{,}631.46 - 56.57(1923.0) \\
&\quad - \{ - 2.49(317.0) - 1.69(277.3) + \ldots - 1.79(276.9)\} \\
&\quad - \{ - 1.07(333.0) -1.59(276.9) - \ldots - .32(337.5)\} \\
&\quad - \{ - 4.05(308.6) - .35(281.7) - \ldots +5.48(369.5)\} \\
&\qquad = 1155.2864
\end{aligned} \tag{106}$$

if the thing be carried out with 4-figure accuracy, of the estimates. In order to find the further reduction due to Treatments it is necessary to consider the full model, with all factors, of Equ. (100). The resultant estimates are:

$$\left.\begin{array}{cccccc}
\multicolumn{6}{c}{\hat{\mu} = 56.78} \\
\hat{\alpha}_1 & \hat{\alpha}_2 & \hat{\alpha}_3 & \hat{\alpha}_4 & \hat{\alpha}_5 & \hat{\alpha}_6 \\
\hline
-2.67 & -.95 & +.78 & +3.73 & +.90 & -1.80 \\
\hat{\beta}_1 & \hat{\beta}_2 & \hat{\beta}_3 & \hat{\beta}_4 & \hat{\beta}_5 & \hat{\beta}_6 \\
\hline
-1.28 & -.72 & -1.81 & +2.11 & +2.23 & -.53
\end{array}\right\} \tag{107}$$

and estimates of $\hat{\gamma}_k$ (or $(\hat{k})$) and $\hat{\delta}_\ell$ (or $[\hat{\ell}]$) as shown in Table XXV c. The residual is

$$\begin{aligned}
&110{,}631.46 - 56.78(1923.0) \\
&\quad - \{ - 2.67(317.0) - .95(277.3) + \ldots - 1.80(276.9)\} \\
&\quad - \{ - 1.28(333.0) - .72(276.9) - \ldots - .53(337.5)\} \\
&\quad - \{ - 3.86(317.5) - .06(340.3) - \ldots +4.72(369.0)\} \\
&\quad - \{ - 4.28(308.6) + .38(281.7) - \ldots + 5.24(369.5)\} \\
&\qquad =666.1826
\end{aligned} \tag{108}$$

Table XXV - Satisfaction of 6 Groups over 6 Weeks with 6 Treatments when 2 observations are missing

a. Data from Table VI with 2 observations supposed missing

		Week							
Group	0	1	2	3	4	5	6	Sum	Mean
I	((1))	(1)46.4[1]	(3)45.8[1]	(2)40.8[3]	(5)62.4[2]	(6)59.9[5]	(4)61.7[6]	317.0	52.8
II	((2))	(2)60.9[2]	(4) NR [2]	(3)44.9[4]	(6)64.2[3]	(1)55.3[6]	(5)52.0[1]	277.3	55.5
III	((3))	(3)50.0[3]	(5)50.0[3]	(4)64.2[5]	(1)60.9[4]	(2)58.4[1]	(6)53.3[2]	336.8	56.1
IV	((4))	(4)63.7[4]	(6)72.0[4]	(5)71.7[6]	(2)57.3[5]	(3)53.3[2]	(1)52.7[3]	370.7	61.8
V	((5))	(5)48.8[5]	(1)50.4[5]	(6)56.4[1]	(3)58.9[6]	(4)65.6[3]	(2)64.2[4]	344.3	57.4
VI	((6))	(6)63.2[6]	(2)58.7[6]	(1)51.8[2]	(4)49.6[1]	(5) NR [4]	(3)53.6[5]	276.9	55.4
Sum		333.0	276.9	329.8	353.3	292.5	337.5	1923.0	

b. Analysis for direct Treatment alone

	$(\hat{1})$	$(\hat{2})$	$(\hat{3})$	$(\hat{4})$	$(\hat{5})$	$(\hat{6})$
Contr.	-3.77	+.03	-5.60	+4.05	+.47	+4.82
Adj. Mn.	52.91	56.71	51.08	60.73	57.15	61.50

Factors	d.f.	Residual Variability (Squares)
$\hat{\mu}$, Rows & Columns (control)	23	1523.52
Control factors & Treatments	18	1034.80

$$F_{5,18} = \frac{(1523.52 - 1034.80)/5}{1034.80/18} = 1.70 \text{ N.S.}$$

c. Analysis for direct Treatment, with allowance for Carry-over

	$(\hat{1})$	$(\hat{2})$	$(\hat{3})$	$(\hat{4})$	$(\hat{5})$	$(\hat{6})$
Contr.	-3.86	-.06	-5.69	+3.93	+.96	+4.72
Adj. Mn.	52.92	56.72	51.09	60.71	57.74	61.50

$[\hat{1}]$	$[\hat{2}]$	$[\hat{2}]$	$[\hat{4}]$	$[\hat{5}]$	$[\hat{6}]$
-4.28	+.38	-3.32	+3.36	-1.39	+5.24

Factors	d.f.	Residual Variability (Squares)
$\hat{\mu}$, Rows, Columns & Carries-over (control)	18	1155.29
Control factors plus Treatments	13	666.18

$$F_{5,13} = \frac{(1155.29 - 666.18)/5}{666.18/13} = 1.91 \text{ N.S.}$$

d. Data arranged by Treatment and by Treatment of the preceding Week

	Treatment								
After	(1)	(2)	(3)	(4)	(5)	(6)	Sum	Adj. Mean	Contr.
(1)	46.4	58.4	45.8	49.6	52.0	56.4	308.6	51.4	-5.5
(2)	51.8	60.9	53.3		62.4	53.3	281.7	57.2	+.3
(3)	52.7	40.8	50.0	65.6	50.0	64.2	323.3	53.9	-3.0
(4)	60.9	64.2	44.9	63.7		72.0	305.7	61.4	+4.5
(5)	50.4	57.3	53.6	64.2	48.8	59.9	334.2	55.7	-1.2
(6)	55.3	58.7	58.9	61.7	71.7	63.2	369.5	61.6	+4.7
Sum	317.5	340.3	306.5	304.8	284.9	369.0	1923.0		
Adj.Mn.	52.9	56.7	51.1	61.0	57.9	61.5			
Contr.	-4.0	-.2	-5.8	+4.1	+1.0	+4.6		56.9	

if the thing be carried out with 4-figure accuracy, of the estimates. These results are summarized in Table XXV c; the value F fails significance, as it did in the full, original table of data.

Unfortunately, one cannot, as for previous problems, in practice avoid the effort required to set up the foregoing equations by having an electronic computer do so, after it has been given the Design and the data, in the electronic program of the Appendix. The problem is discussed there. Briefly, the program would have to be extended.

In the situation of missing cells rather than Rows, the procedure of breaking variability up is exactly as formerly. Again, in the test, there tends to be a slight exaggeration of significance. This is again because the estimates of Treatment effect vary slightly in their reliability, depending on how many observations are involved in each.

In Table XXV the number of degrees of freedom left after the control and experimental factors have been handled is the number of observations less the number of independent constants estimated. These factors are, of course, exactly the same as in the full Design so the freedom assigned them is also the same. The loss in freedom is confined to the residual variability. The test of significance is again slightly upset by all Treatments not appearing an equal number of times. We shall again judge the value of F a little the more startling than is proper. To put the matter another way, it is as if Table L gave us a value a little too low. By this it is meant that the 5% or 1% level of significance should be slightly higher than it comes from Table L.

It may be of some interest to note that in Table XXV the adjusted mean for a Treatment, i.e., $\hat{\gamma}_k + \hat{\mu}$, is the same as the corresponding value in Table XIX, without allowance for Carry-over, or in Table XXI, with allowance for Carry-over, for $k = (1),(2),(3)$ or (6). These are Treatments that lack no observation; the information on them is not affected by the misses and their average is unchanged.

VI. Analysis of data from a single Youden, t x c x t

Page

The general nature of data from a Youden rectangle 181

Estimation of direct treatment effects only in a single Youden rectangle 183

Analysis of significance for direct treatment effects alone . . . 187

Breaking treatment variability up 200

Analysis with allowance for Carry-over in Youden rectangles . . . 203

Explicit solution for simple cases of Carry-over 217

Youden rectangles with missing Rows 219

Youden rectangles with missing cells 228

The general nature of data from a Youden rectangle - We must consider the problem of dealing with data from a single Youden, $c < t$, in the light of discussion of data from a latin square, as in the just previous Chap. V. The necessity arises, in part, from the fact that all biometrical analysis is thus written; it is dominated by the forms and concepts of the latin square or at least its degenerate form the balanced block. It arises, in the second part, because a great many readers must be already familiar with such analysis and so it is wise both to take advantage of that familiarity and to warn against it. Youden rectangles differ from the squares first in that the nature of the analysis is not so obvious. The Treatments are heavily confounded with Rows so that it is not enough to look over some data and say "well, Treatment (7) is the highest, anyhow." It does not necessarily follow that (7) is best because it may simply have happened that Treatment (7) was assigned to Rows that proved high almost regardless of the Treatment put into them. The analysis of variance with which so many of us are extremely familiar breaks down. Since the Rows and Treatments are not orthogonal the sum of squares to be ascribed to each depends on the order of their consideration. The matter may be more clear if we think of it in terms of the reduction of residual variability (or squares). Then the reduction in residual variability for Treatments is one thing if that for Rows is made first but quite another thing if the reduction for Treatments is made first and then that for Rows. In any case the sum of squares of the usual analysis of variance does not on division by the degrees of freedom yield in any clear and assured way the mean square which may be tested for error. The literature on the subject of analysing Youden rectangles consists of data writhing on the Procrustean analysis of variance.

The difficulties to which allusion has just been made obtain for a Youden rectangle when one is simply trying to deal with direct treatment effects without any complication of Carry-over. When this type of analysis is extended to embrace carry-over effects, the contortions are indeed dreadful; such efforts are enough to disengage people from this admirable experimental point of view. Perhaps that is why it has been used so little. As an example, Patterson and Lucas (1962) in their learned review of change-over experiments attempt to force the analysis of their data into the form of an analysis of variance. A reform of view-point helps. The development of electronic computers that can set up equations and then solve them very rapidly is also a help. Perhaps new times are at hand and we shall use change-over Youden rectangles, making allowance for Carry-over, very freely and discover many things that will help the fields of application that are involved.

For the Youden rectangle it is best to follow the treatment, alternative to classical analysis of variance, as for the latin square just discussed, and which is recommended for the more complicated cases. In general, we set up the least squares equations for the model involving all things likely to control variability, i.e., Rows and Columns and get the residual variability. Then we extend the model to include Treatments and again find residual variability and reduction in variability. This we consider against the variability residual on the total model, with due allowance for degrees of freedom strike the F ratio and decide on significance.

For single Youdens it is possible to have a 0^{th} conditioning Period of the same Treatment as the 1^{st} time experimental Period, just as has been shown previously for the latin square. It is not, however, particularly profitable because the Change-over is still not balanced. In a Youden, a given Treatment fails to be preceded by other Treatments than itself. Accordingly, all the Youden rectangles, $c < t$, that follow are unconditioning Design.

Estimation of direct treatment effects only in a single Youden rectangle - The comparatively simple question of direct treatment effects in the single complete Youden rectangle, $t \times c \times t$, $c < t$, will be considered alone for two reasons. First, it is amenable to explicit solution. Secondly, the related problem of testing the significance of treatment effects can be handled in so many ways and is so heavily involved with the literature that it is discussed separately in the following section. As an example of the results gotten by running a single Youden, consider Table XXVI, when the Result was pounds of steel produced under 7 Oils which were tried on each of 7 Machines over 4 Periods of operation, i.e., 7x4x7. The Treatments are disposed according to the Design shown in Table XIII. The circumstance that this Design is arranged with cyclic Columns is immaterial in analysis for direct effects, without consideration of Carry-over. The present discussion applies to Youden rectangles in general.

As previously, for the latin square, we may say we get the Result, y_{ijk} , in the i^{th} Row, i.e., Machine, in the j^{th} Column, i.e., Period, under the k^{th} Treatment, following the model of Equ. (7). Again, it may be observed that the business of getting a sum of all observations y_{ijk} relevant to an effect such as that of Oil or Treatment (1), plus such irrelevant quantities as are unavoidable, and then the averaging of the values y_{ijk} is really the essence of the method of least squares. It is necessary to turn from the total effect of a Treatment such as (1) to some sort of average effect, that is, the amount such a Treatment may be said to affect a single observation y_{ijk} subject to it. It is necessary to remove, as best possible, the irrelevant contributions. Since each Treatment occurs in all Columns, these two effects are not confounded, but, of course, only some Treatments can occur in a given Row so that there confounding does occur. If the sum for each Treatment is formed there result the equations,

$$\left.\begin{array}{l}
4\hat{\mu} - \hat{\alpha}_3 - \hat{\alpha}_4 - \hat{\alpha}_6 + 4\hat{\gamma}_1 = 1339.4 \\
4\hat{\mu} - \hat{\alpha}_4 - \hat{\alpha}_5 - \hat{\alpha}_7 + 4\hat{\gamma}_2 = 1289.0 \\
4\hat{\mu} - \hat{\alpha}_5 - \hat{\alpha}_6 - \hat{\alpha}_1 + 4\hat{\gamma}_3 = 1509.4 \\
\cdots\cdots\cdots\cdots\cdots\cdots \\
4\hat{\mu} - \hat{\alpha}_2 - \hat{\alpha}_3 - \hat{\alpha}_5 + 4\hat{\gamma}_7 = 1523.1
\end{array}\right\} \tag{109}$$

Accordingly it is necessary to find the sums, or least squares equations for Rows, i.e.,

$$\left.\begin{array}{l}
4\hat{\mu} + 4\hat{\alpha}_1 - \hat{\gamma}_3 - \hat{\gamma}_5 - \hat{\gamma}_6 = 1205.5 \\
4\hat{\mu} + 4\hat{\alpha}_2 - \hat{\gamma}_4 - \hat{\gamma}_6 - \hat{\gamma}_7 = 1497.5 \\
4\hat{\mu} + 4\hat{\alpha}_3 - \hat{\gamma}_5 - \hat{\gamma}_7 - \hat{\gamma}_1 = 1221.7 \\
\cdots\cdots\cdots\cdots\cdots\cdots \\
4\hat{\mu} + 4\hat{\alpha}_7 - \hat{\gamma}_2 - \hat{\gamma}_4 - \hat{\gamma}_5 = 1526.2
\end{array}\right\} \tag{110}$$

and deal with the general constant $\hat{\mu}$. We must find the overall total and write

$$\left.\begin{array}{r}
28\hat{\mu} = 10{,}244.4 \\
\hat{\mu} = 365.9
\end{array}\right\} . \tag{111}$$

The simplification is due to the fact that in the overall total all Rows are represented equally (7 times) and due to Equ. (9), therefore, come to naught. Similarly the Column totals come to naught. The solution of these equations provides the estimates of Treatment effects shown in Table XXVIb.

So long as one is dealing, as we are here, with perfect and regular Youden rectangles, one may estimate the effect of the k^{th} Treatment or Oil explicitly as

$$\hat{\gamma}_k \text{ or } (\hat{k}) = \frac{c\Sigma y_{ijk} - \Sigma T_i}{ft} \qquad (112)$$

where y_{ijk} is any observation under Oil k and where T_i is the sum of the observations for any Row in which k occur. Thus the estimate of the effect of an Oil (1) is, following Equ. (112) from Table XXVI:

$$\begin{aligned}\gamma_1 \text{ or } (\hat{1}) &= \{4(272.9) - 1205.5 + 4(316.4) - 1497.5 + 4(412.2) - 1644.3 + \\ &\quad 4(337.9) - 1526.2\}/14 \\ &= -36.8 \quad . \qquad (113)\end{aligned}$$

The estimates gotten in this way of the effect of all Oils are as shown in Table XXVIb. As a numerical check the sum of the estimates, of course, total to zero. The estimates from Equ. (112) are least squares estimates and as such can be later applied in the equations, where we deal with residual variability. The Youden conditions not only give a powerful comparison of Treatments, in the sense of the comparison table as in Tables I, II and III, but the very tidy set of least squares equations (109), (110) and (111), which lead to the simple explicit result of Equ. (112). This is true, of course, only so long as the Design is exactly complete, without missing or extra Rows, "but this happeneth rarely." From the experimental point of view Equ. (112) is interesting because it shows clearly that the rating put on a Treatment or

Oil is indeed not its total response but that total adjusted, as is intuitively attractive, for the Row or Machine where it occurs.

The quantities shown in Table XXVIb as Adjusted Mean were formed by adding, for each Treatment, $(\hat{k})$ to $\hat{\mu}$. They are comparable to the true means from a latin square in that they average to $\hat{\mu}$ and yield the contributions when that is subtracted from each of them. They are essentially estimates of what the mean would have been if a given Treatment had been tried in all Rows. They are very useful quantities in reporting work because they are necessarily of the correct magnitude for whatever phenomenon is under consideration.

Let us consider first the method of squeezing the problem into the form of the analysis of variance which appears widely in the standard literature. It all gives much the same analysis and draws very much the same conclusions. Let us consider the discussion of Cochran and Cox (1957: 509) in some detail. We are concerned with their "Analysis with recovery of inter-block information," not at all with the alternative restricted to intra-block variability. The latter is a slightly more simple variant that is more or less admissible under certain circumstances. For a single Youden with t = number of Treatments, c = number of Columns and r = number of Rows, the procedure is:

a. Find column totals, block totals and corner or grand total G.

b. Write a column of treatment totals as T_k $(k=1..t)$, i.e., the total for all observation under Treatment k.

c. In a column beside that of T_k write a column of S_k, the total of row sums T_i for all Rows in which a given kth Treatment occur.

d. Write a 4th column of the quantities.

$$W_k = (t-c)\ T_k - (t-1)\ S_k + (c-1)\ G \qquad (114)$$

e. Write the analysis of sums of squares and mean squares:

Source	d.f.	Sum Squares	Mean Square
Rows (adjusted)	t-1	S'_R	$E_R=S'_R/(t-1)$
Columns	c-1	S_C	
Treatments (unadjusted)	t-1	S_T	
Residuals	(c-2)(t-1)	S'_E	$E_E=S'_E/(c-2)(t-1)$
"Total"	tc-1	S_G	

Here sums of squares for "Total," for Columns and for Treatments are found by the usual methods, without consideration of the Design, i.e., from Equ. (13) and (14). The adjusted sum of squares for Rows is the sum of squares of the quantities W_k divided by $ct(t - c)(c - 1)$. Sum of squares for error is gotten by subtraction, in a fashion after Equ. (15).

f. Calculate the quantity

$$M = \frac{E_R - E_E}{t(c - 1)E_R} \qquad (115)$$

and then the adjusted mean for the k^{th} Treatment is

$$\hat{(k)} = T_k - MW_k \qquad (116)$$

which is, of course, a restatement of Equ. (112).

g. Finally, a method is given for testing the significance of the difference between such estimates for any two Treatments. Into this it seems wisest not to go.

Analysis of significance for direct treatment effects alone - When it comes to the analysis of significance for direct Treatment alone, even though the Design may be a Change-over, one is simply dealing with a Youden rectangle in a form on which there exists a considerable literature. This cannot be ignored, even though it does not seem so handy, as it might be, to the writer. This corpus will be first considered because people may still test significance by

Table XXVI - Pounds of steel, by 7 Machines over 4 Periods with 7 experimental Oils

a. Data

Machine	Period 1	2	3	4	Sum
I	(1)272.9	(2)302.4	(4)349.1	(7)281.1	1,205.5
II	(2)384.4	(3)424.7	(5)372.0	(1)316.4	1,497.5
III	(3)292.9	(4)356.4	(6)374.2	(2)198.2	1,221.7
IV	(4)530.2	(5)425.2	(7)309.9	(3)464.7	1,730.0
V	(5)304.1	(6)510.6	(1)412.2	(4)417.4	1,644.3
VI	(6)319.0	(7)474.3	(2)404.0	(5)221.9	1,419.2
VII	(7)457.8	(1)337.9	(3)327.1	(6)403.4	1,526.2
Sum	2,561.3	2,831.5	2,548.5	2,303.1	10,244.4
Sum sq.	992,958.87	1,179,481.11	936,437.71	820,549.43	

b. Treatment (Oils) estimates

	$(\hat{1})$	$(\hat{2})$	$(\hat{3})$	$(\hat{4})$	$(\hat{5})$	$(\hat{6})$	$(\hat{7})$
Contr.	-36.8	-13.4	+4.4	+57.9	-71.3	+44.1	+15.1
Adj. Mn.	329.1	352.5	370.3	423.8	294.6	410.0	381.0

using a desk calculator, and this is quite practical if one has only a single and perfect Youden rectangle, without consideration of possible Carry-over. It has the further advantage of being logically available to people used to thinking in terms of analysis of variance. Secondly, from essentially the same viewpoint certain modifications will be suggested. They again apply to the single and perfect Youden rectangle, without allowance for possible Carry-over and are entirely practical on a desk calculator. Finally, there will be presented a third handling written in terms of least squares equations and variability residual from them--all this just as has been done immediately previously for latin squares. This last technique is preferable in that it disdains the singleness of perfection of the Youden rectangle. It can, of course, and will be extended to the situation where allowance is made for possible Carry-over. The reader, unacquainted with the literature and essentially inclined to getting a job done, is advised to skip to this third general method of handling the data.

Let us, for the moment, illustrate the foregoing discussion on the data of Table XXVI, the 7x4x7, on Machines and Oils. This is done in Table XXVIIa. In particular we note that Equ. (114) becomes

$$W_k = 3T_k - 6S_k + 3G \qquad (117)$$

The adjusted sum of squares for Rows is

$$\{(-489.6)^2 + (+2,536.8)^2 + \dots + (+17.1)^2\}/(4)(7)(3)(3)$$
$$= 69,592.93 \ . \qquad (118)$$

So far as the other sums of squares, which are simply those gotten from any standard analysis of variance, are concerned they are from Equ. (13),

$$S_C = \{4[(2,561.3)^2 + \ldots + (2,303.1)^2] - (10,244.4)^2\}/28$$
$$= 19,976.99 \qquad (119)$$

and from Equ. (14),

$$S_G = \{28[(272.9)^2 + (302.4)^2 + \ldots + (403.4)^2] - (10,244.4)^2\}/28$$
$$= 181,293.86\ . \qquad (120)$$

The value of S_E' is gotten by differencing. For the sake of convenience there appears in the same Table a part XXVIIb which is an alternate form of the analysis of variance as will be discussed immediately and XXVIIc, which is the corresponding analysis on the basis of least squares equations. The latter will be discussed in due course.

It should be noted that two operations are going on in the above procedure The quantities that we have called sum of squares are found so that we can conveniently estimate error sum of squares by the subtraction of Periods and Machines from total variability. They are found for this purpose and nothing else. The sum of squares for something like Rows is simply the reduction in total variability achieved by the consideration of Rows. The quantities called mean squares are found so that we can test significance. Thus the mean square due to Rows is the estimate that we should make from among the Rows of the unit variability if the data were to consist of random normal deviates. These two kinds of quantities,sums of squares and mean squares, are not necessarily related in a simple way. The misleading circumstance is that they are simply related in the less involved Designs such as latin squares. There all sums of squares are divided by comparatively simple associated integers called degrees of freedom to yield mean squares suitable for testing if necessary. This has been so much the case that the widely known analysis of variance is thought of as fundamental. In the development of statistical analysis there

Table XXVII- Analysis of variance for Youden, 7x4x7, without Carry-over.

Data of Table XXVI

a. Analysis following the literature

Treatment	T_k	S_k	W_k
(1)	1,339.4	5,873.5	- 489.6
(2)	1,289.0	5,343.9	+2,536.8
(3)	1,509.4	5,975.4	- 591.0
(4)	1,653.1	5,801.5	+ 883.5
(5)	1,323.2	6,291.0	-3,043.2
(6)	1,607.2	5,811.4	+ 686.4
(7)	1,523.1	5,880.9	+ 17.1
Sum.	10,244.4	40,977.6	.0

Source	d.f.	Sum Squares	Mean Square	F
Rows (adj)	6	69,592.93	11,598.82	2.33 N.S.
Columns or Periods	3	19,976.99	6,658.93	1.34 N.S.
Treatments or Oils (unadj)	6	31,947.99	--------	
Residuals	12	59,775.95	4,981.33	
"Total"	27	181,293.86		

b. Analysis when the roles of Rows and Treatments are interchanged

Treat-ment	Period								Sum
	1		2		3		4		
(1)	I	272.9	VII	337.9	V	412.2	II	316.4	1,339.4
(2)	II	384.4	I	302.4	VI	404.0	III	198.2	1,289.0
(3)	III	292.9	II	424.7	VII	327.1	IV	464.7	1,509.4
(4)	IV	530.2	III	356.4	I	349.1	V	417.4	1,653.1
(5)	V	304.1	IV	425.2	II	372.0	VI	221.9	1,323.2
(6)	VI	319.0	V	510.6	III	374.2	VII	403.4	1,607.2
(7)	VII	457.8	VI	474.3	IV	309.9	I	281.1	1,523.1
Sum		2,561.3		2,831.5		2,548.5		2,303.1	10,244.4

Row	T'_i	S'_i	W'_i
I	1,205.5	5,804.6	-477.9
II	1,497.5	5,461.0	+2,459.7
III	1,221.7	6,058.7	-1,953.9
IV	1,730.0	6,008.8	-129.6
V	1,644.3	5,922.9	+128.7
VI	1,419.2	5,742.5	+535.8
VII	1,526.2	5,979.1	-562.8
Sum	10,244.4	40,977.6	.0

Source	d.f.	Sum Squares	Mean Square	F
Rows or Machines (unadj)	6	58,947.98	--------	
Columns or Periods	3	19,976.99	6,658.93	1.34 N.S.
Treatments or Oils (adj)	6	42,592.94	7,098.82	1.42 N.S.
Residuals	12	59,775.95	4,981.31	
"Total"	27	181,293.86		

c. Analysis directly from least squares equations

Factors	d.f.	Residual Variability (Squares)
$\hat{\mu}$, Rows and Columns (control)	18	102,368.89
Control factors plus Treatments	12	59,775.94

$$F_{6,12} = \frac{(102{,}368.89 - 59{,}775.94)/6}{59{,}775.94/12} = 1.43 \quad \text{N.S.}$$

has arisen a confusion of realities and means. The systematic association of sums of squares and mean squares is well enough for a situation without systematic confounding but stands us in poor stead now. This confusion arising from form is hard to shake since it comes from the Olympian pen of Sir Ronald Fisher. It is also hard, of course, because the point of view of the analysis of variance remains extremely useful in certain connections, such as that of breaking up the variability among Treatments. It should be understood that the distinction between sums of squares and mean squares here made is the view of the writer and will not be found generally in the literature. In connection with the procedure that we recommend for the test of significance of Treatments in the single Youden rectangle note that in this problem there is no simple relationship between elements that play the classic role of sums of squares in the analysis of variance and elements that play the role of mean squares. It is the attempts in the literature to force such a relationship that make the analysis of single Youdens so hard to read and harder still to follow.

The preceding approach may be bettered in two ways of which the first is that of inverting the roles of Rows and Treatments. A curious, and at first glance apparently trivial, circumstance is that if one rearrange the data, y_{ijk} , of a Youden Design, t x c x t , where entry is by, say, Subjects and Periods, to entry by Treatments and Periods, i.e., to values y_{kji} , the Design remains Youden,* t x c x t . To illustrate what is meant consider the Design, in cyclic Columns, for 7x4x7, as follows:

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*Beall, G. Reversion of a Youden rectangle. Educational Testing Service, Princeton, N. J. (in process, 1971).

Row	Period			
	1	2	3	4
I	(1)	(2)	(4)	(7)
II	(2)	(3)	(5)	(1)
III	(3)	(4)	(6)	(2)
IV	(4)	(5)	(7)	(3)
V	(5)	(6)	(1)	(4)
VI	(6)	(7)	(2)	(5)
VII	(7)	(1)	(3)	(6)

Treatment	Period			
	1	2	3	4
(1)	I	VII	V	II
(2)	II	I	VI	III
(3)	III	II	VII	IV
(4)	IV	III	I	V
(5)	V	IV	II	VI
(6)	VI	V	III	VII
(7)	VII	VI	IV	I

The second Design will be found Youden, in its way. This interchangeability of role of experimental Subject and Treatment may be employed to make estimates of the effects, $\hat{\alpha}_1, \hat{\alpha}_2, \ldots, \hat{\alpha}_7$, for Row or Machine. In strict analogy to the estimates, $\hat{(1)}, \hat{(2)}, \ldots, \hat{(7)}$, for the Oils, from Equ. (112), the estimate for the i^{th} Machine is

$$\hat{\alpha}_i = \frac{c\Sigma y_{kji} - \Sigma T_k}{ft} \tag{121}$$

where y_{kji} is any observation from Machine i and where T_k is the total for any line in the inverted table (involving now, of course, a given Oil) in which i occur. Thus the estimate for Machine I, of the data of Table XXVI, is

$$\{4(272.9) - 1339.4 + 4(302.4) - 1289.0 + 4(349.1) - 1653.1 + 4(281.1) - 1523.1\}/14$$
$$= - 70.2 \quad . \tag{122}$$

Further, one can very simply rearrange matters so that Treatments rather than Rows are adjusted in the analysis of variance, and their mean square can then be tested for significance. Thus like Equ. (114),

$$W_i' = 3T_i' - 6S_i' + 3G \quad , \tag{123}$$

with the roles of Rows and Treatments reversed. So matters would be much mended. This has been done in Table XXVIIb. So far as the sum of squares (unadjusted) for Rows is concerned, this is now, simply, from Equ. (13),

$$S_R = \{7[(1,205.5)^2 + \ldots + (1,526.2)^2] - (10,244.4)^2\}/28$$
$$= 58,947.98 \qquad (124)$$

The sum of squares for Rows and Columns, respectively, and that for "Total" may be gotten in various ways, one of which might be by classical analysis of variance (omitting Treatment) as from Equ. (13).

The analysis of variance, from the literature and as illustrated in Table XXVIIa, with its adjusted row effects and unadjusted treatment effects, seems aimed principally at getting a scheme such that the sum of squares for Residuals can be gotten by subtraction. The mean square for Treatments cannot be tested for significance against the Residuals, which seems a pity. There is not, explicitly, an F test among Treatments in general. The mean square for Rows can be tested, by the usual ratio method, against Residuals although this seems an idle thing. There is little point in showing that one's experimental arrangement was worthwhile. Matters can be arranged more profitably as just discussed and as illustrated in Table XXVIIb. Now the adjusted sum of squares for Treatments can be tested against residual variability and this is a thing worth doing, if it is worthwhile at all to make a test of significance.

It may be noted that the analysis of variance in Table XXVIIb is, in a fundamental way, identical with the analysis proposed in the previous section, on least square equations. The "Total" is the sum of squares reduced by the estimate of $\hat{\mu}$. The sums of squares for Rows and Columns are the reductions due to the control factors, without any allowance for Treatment. Finally, the sum of squares for Treatments (adjusted) is the further reduction in the sum of squares due to their introduction into the model.

Granted that the analysis may be bettered by testing the Treatments rather than the Rows, a second betterment is to simplify the calculation of the sum of squares and mean square for Treatments (adjusted), as in Table XXVIIb, by using a relationship that seems to have been generally overlooked, viz., that the sum of squares for Treatments (adjusted) is*

$$S'_T = \frac{(c - 1)t}{t - 1} \sum_t (\hat{k})^2 \qquad (125)$$

which can be calculated very simply from the treatment estimates $\hat{\gamma}_k$ or $(\hat{k})$ as in Equ. (112). The mean square is

$$E_T = \frac{(c - 1)t}{(t - 1)^2} \sum_t (\hat{k})^2 \quad . \qquad (126)$$

Thus one avoids the complicated little sub-table of T'_i, S'_i and W'_i of Table XXVIIb. Then the analysis of variance becomes fairly practical. The present discussion may be illustrated on the data of Table XXVI. We have from Equ. (125) with the results of Table XXVIb

$$\begin{aligned} S'_T &= \frac{3(7)}{6} \left[(-36{:}8)^2 + (-13.4)^2 + \ldots + (+15.1)^2 \right] \\ &= 42{,}567.28 \end{aligned} \qquad (127)$$

or, if the estimates, $(\hat{k})$, are calculated with more decimal places 42,592.94, as shown in the analysis of sums of squares of Table XXVIIb.

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*Beall, G. On finding the adjusted sum of squares for Treatments in a Youden rectangle. Educational Testing Service, Princeton, N. J. (in process 1971).

If one should want a mean square for Machines that may properly be compared with Residual it may also be gotten, parallel to Equ. (125) from the row or machine estimates of results such as Equ.(135), in the form

$$E_R = \frac{(c - 1)t}{(t - 1)^2} \sum_t \hat{\alpha}_i^2 \qquad (128)$$

or for the data of Table XXVI, as

$$7[(-70.2)^2 + (+37.8)^2 +\ldots+(+9.0)^2]/12 = 11,599.33 \qquad (129)$$

and this may be used with the Residuals mean square of Table XXVIIa or b, to strike an F ratio.

As originally stated, an alternative to the methods of analysis of variance just reported there is the method of working with least squares equations. This is still preferable to using analysis of variance in that the former will fit in with the necessity of going on to more elaborate models involving repeated Design, missing data, Carry-over etc. The former has generality. It is not immediately suitable for working with a desk calculator, in many cases. It works out admirably with an electronic computer, as in the Appendix program. It can also be used in what is termed a hybrid procedure at the termination of the present section. For a single Youden rectangle it is necessary to set up the least squares equations by forming the rather obvious sums for all observations involving a given effect and then solve the equations on an electronic computer, if that be available. The best way, if matters stand favorably, is simply to give the Design and data to the appropriate appendix program and let it work the whole matter out. This procedure is exactly the same, in principle, as for the latin square.

For the data of Table XXVI, to test the significance of treatment effects, it is first necessary to find the variability residual on the control factors of $\hat{\mu}$, Rows and Columns. The estimate for $\hat{\mu}$ is as in Equ.(111). Forming totals for the Rows, we may say

$$\left.\begin{array}{l} 4\hat{\mu} + 4\hat{\alpha}_1 = 1205.5 \\ 4\hat{\mu} + 4\hat{\alpha}_2 = 1497.5 \\ 4\hat{\mu} + 4\hat{\alpha}_3 = 1221.7 \\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot \\ 4\hat{\mu} + 4\hat{\alpha}_7 = 1526.2 \end{array}\right\} . \qquad (130)$$

Accordingly, we may say that before allowance is made for treatment effects the row effects are:

$$\begin{array}{ccccccc} \hat{\alpha}_1 & \hat{\alpha}_2 & \hat{\alpha}_3 & \hat{\alpha}_4 & \hat{\alpha}_5 & \hat{\alpha}_6 & \hat{\alpha}_7 \\ \hline -64.5 & +8.5 & -60.4 & +66.6 & +45.2 & -11.1 & +15.7 \end{array} . \qquad (131)$$

Similarly for Columns

$$\left.\begin{array}{l} 7\hat{\mu} + 7\hat{\beta}_1 = 2,561.3 \\ 7\hat{\mu} + 7\hat{\beta}_2 = 2,831.5 \\ 7\hat{\mu} + 7\hat{\beta}_3 = 2,548.5 \\ 7\hat{\mu} + 7\hat{\beta}_4 = 2,303.1 \end{array}\right\} \qquad (132)$$

and

$$\begin{array}{cccc} \hat{\beta}_1 & \hat{\beta}_2 & \hat{\beta}_3 & \hat{\beta}_4 \\ \hline +.0 & +38.6 & -1.8 & -36.9 \end{array} . \qquad (133)$$

Accordingly we find the residual variability (squares) on the control factors is:

$$\begin{aligned}
&3{,}929{,}427.12 - 365.87(10{,}244.4) \\
&- \{-64.5(1205.5) + 8.5(1497.5) \ldots + 15.7(1526.2)\} \\
&- \{+.0(2561.3) + 38.6(2831.5) - 1.8(2548.5) - 36.9(2303.1)\} \\
&= 102{,}368.89
\end{aligned} \qquad (134)$$

as shown in Table XXVIIc.

For contrast with the foregoing it is necessary to estimate the constants and find the residual squares when Treatment is added, as in Equ. (43), to the model, over and above the control factors. The equations for Rows have to be rewritten, of course, because Treatment is confounded with Rows, i.e., is not balanced from Row to Row. Accordingly, we must replace Equ. (130) by Equ.(110), as previously, or on the lines of Equ. (122), to get estimates for Rows as follows:

$$\begin{array}{ccccccc}
\hat{\alpha}_1 & \hat{\alpha}_2 & \hat{\alpha}_3 & \hat{\alpha}_4 & \hat{\alpha}_5 & \hat{\alpha}_6 & \hat{\alpha}_7 \\
\hline
-70.2 & +37.8 & -83.7 & +65.1 & +46.7 & -4.7 & +9.0
\end{array} \;. \qquad (135)$$

We must complete with the estimates for Treatments (Oils) shown in Table XXVIb, which were calculated from Equ. (109). Using all the estimates and right-hand members of Equ. (111), (132), (110) and (109) we get residual squares of

$$\begin{aligned}
&3{,}929{,}427.12 - 365.87(10{,}244.4) \\
&- \{-70.2(1205.5) + 37.8(1497.5) \ldots + 9.0(1526.2)\} \\
&- \{+.0(2561.3) + 38.6(2831.5) - 1.8(2548.5) - 36.9(2303.1)\} \\
&- \{-36.8(1339.4) - 13.4(1289.0) \ldots + 15.1(1523.1)\} \\
&= 59{,}775.94
\end{aligned} \qquad (136)$$

(actually using the coefficients with accuracy to the 6th decimal place) as shown in Table XXVIIc. The complete factor analysis, control and experimental, which corresponds to the classic analysis of variance is shown there. In practice, it can, of course, be calculated directly by simply feeding the Design and data into the appropriate appendix program for an electronic computer.

There is finally what may be called a hybrid method of calculating significance that is highly practical for a desk calculator when one is working with a perfect,single,Youden rectangle. The method consists of employing the explicit solutions for Rows, as in Equ.(121) for Treatments, as in Equ.(136) and, of course, for Columns from Equ. (132), against the appropriate right-hand members, or sums of the simultaneous equations. This avoids the setting up of the left-hand members of the equations, let alone the general solution of the equations. Then all that remains to be done is the calculation of Equ. (134) and (136).

Breaking treatment variability up - Very often in a Youden rectangle, $c < t$, just as in a latin square, $c = t$, it is necessary to consider some particular comparisons from among the effect of Treatments as a whole group. The best procedure is just the same, fundamentally. Work with the Treatment estimates and having broken up correctly the variability among them reconcile the results to the overall discussion by a factor of **K** . Consider the data of Table XXVI on lb. of steel. Suppose it were required to compare Oils (1) and (2) with all other Oils, as a group. Then on the lines of Equ. (36) there is found a factor K , related to the total reduction in squares for Treatments. This is the sum of squares (adjusted) for Treatments in the analysis of variance of Table XXVIIb or the reduction in squares due to Treatments, i.e., 102,368.89 - 59,775.94 = 42,592.95, of Table XXVII c. In any case,

$$K = \frac{42,592.94}{(-36.8)^2 + (-13.4)^2 + (+4.4)^2 + (+57.9)^2 + (-71.3)^2 + (+44.1)^2 + (+15.1)^2}$$

$$= 3.5021 \qquad (137)$$

If this value had been based on estimate of treatment effect with more decimal places, as it would be on calculation not intended for illustration, it would be exactly 3.5. This is plain in the situation of a perfect Youden rectangle from Equ.(125) from which

$$K = \frac{(c - 1)t}{t - 1} = S_T'/\Sigma_k (\hat{k}) \quad . \qquad (138)$$

Now we may say that variability due to (1) + (2) versus the rest of the Treatments is

$$3.5\{ \frac{(-36.8 -13.4)^2}{2} + \frac{(+ 4.4 + 57.9 - 71.3 + 44.1 + 15.1)^2}{4} \}$$

$$= 3.5\{(50.2)^2\}3/4$$

$$= 6615.105 \quad . \qquad (139)$$

Now this value is to be compared with the final residual variability (after effect of Treatments is removed) to yield

$$F_{1,12} = 6615.105/4981.31 = 1.33 \qquad (140)$$

which is not significant. It should be further observed that in the present case where, since the Youden rectangle is perfect, each Treatment is represented the same number of times, the test is exact. There is no need for the demur that was necessary in the case of a latin square with a Row or

Rows missing and that will be necessary, again, for a Youden rectangle, when such omission occurs.

It will be observed that in the immediately foregoing paragraph the variability among Treatments is as easily broken up as it was for the latin square in Chap. V. Here, however, there seems to be no other practical way of doing it while, under peculiar simplifying conditions, there was an alternate and standard way of attending to the matter. It was, however, on account of the generality of the method used in the preceding paragraph that the present method was there introduced. Further, the present method can be extended to even more recalcitrant situations, as will appear later.

Finally, it may be of interest to consider the problem of comparing two treatment means--this is a subject that the standard discussion in the literature stresses. Suppose that for the situation of Table XXVIb, it were required to find the significance of the difference between Treatments $(\hat{2})$ and $(\hat{6})$. Then following Equ. (35) and (137)

$$E = K[(\hat{2})^2 + (\hat{6})^2 - \frac{\{(\hat{2}) + (\hat{6})\}^2}{2}]$$

$$= \frac{K}{2}\{(\hat{2}) - (\hat{6})\}^2$$

$$= 3.5\{ - 13.4 - 44.1\}^2/2$$

$$= 5785.94 \ .$$

Then

$$F_{1,12} = 5785.94/4981.31 = 1.16 \qquad (141)$$

which is not significant.

Analysis with allowance for Carry-over in Youden rectangles - In Chap. II, it was suggested that Carry-over of treatment effects may often be considerable and should be allowed for. On this basis the Designs presented for Youden rectangles have made allowance for Change-over. That is, a given Treatment is not preceded by any other Treatment more than once. With such Designs we may at least comfort ourselves that Carry-over, if it occur, will be confounded in a small and presumably minimal way with treatment effects. The matter often stops there. It may, however, be necessary to free estimates of treatment effects from Carry-over. It may further be required to get Carry-over out of the residual mean square which is blown up by it. There are even times when it is of great practical importance to estimate Change-over itself. Thus, in one way or another we are often under the necessity of analyzing for Change-over, in a manner similar to that in which it was treated in Chap. V, for latin square Design. For the Youden rectangle, however, there will be shown only cases without a conditioning Period. There is not much point to putting in conditioning Periods, because the Carry-over would still not be balanced; in the Youden rectangle a given Treatment is not preceded (or followed) by all other Treatments than itself. The defect is not so simple as in the case of a latin square nor remedied in such a simple way.

In the data of Table XXVIII, used to illustrate the problem of dealing with Carry-over in a Youden rectangle, $c < t$, without conditioning previous Period, it is assumed that the background was at least substantially the same for all Treatments in the first experimental Period, as for the corresponding latin squares previously discussed. Thus for the first Column or Period of a Design, such as that of Table XXVIIIa, it must be supposed that the model of Equ. (7) still obtains, i.e., there is no term for Carry-over. It is supposed uniform for all items in the first Column and hence completely confounded with the effect of the first Column. It is completely confounded with the general effect of the first Column and so for this Column we must simplify our model. Otherwise we proceed as previously for the latin square with a conditioning Period. Equ. (43) is the model obtaining.

An illustration of a Youden rectangle, subject to analysis for Carry-over is provided by an 11x5x11, $f = 2$. Each of 11 Groups of men were subject to one Treatment for a Week and 5 Treatments over 5 Weeks. There were 11 Treatments. Each week each Group yielded a percentage of satisfaction. The Design and results are shown in Table XXVIIIa.

The business of analysis again involves setting up least squares equations for the effects, μ , α_i , β_j , γ_k and δ_ℓ appropriate for various levels of the models of Equ. (7) and Equ. (43) and finding the variability variously residual. The handiest thing to do seems to be to set up the full equations, as in Equ. (48), previously, and then cut back to the lesser situations by judiciously dropping rows and columns. Examining Table XXVIIIa, it is at once apparent that the grand total 3768 contains all effects of Rows, Columns, Treatments and Carries-over equally, so that we may write

$$\begin{aligned} 55\hat{\mu} &= 3768 \\ \hat{\mu} &= 68.51 \end{aligned} \tag{142}$$

which is shown as the first line of the grand set of Equ. (145). When it comes to estimating the effect of Rows, it is necessary to recognize that their totals contain no effect of Column, in the sense that they contain them all equally and remembering Equ. (9). On the other hand the total for Row contains much effect of Treatment. Thus for Row, or Group I, there are present Treatments (1), (2), (4), (7) and (11). It is no gain to convert this statement into one that it lacks Treatments (3), (5), (6), (8), (9) and (10). The total for Row I must also contain 4 Carries-over. Thus there may be written

$$5\hat{\mu} + 5\hat{\alpha}_1 + \hat{\gamma}_1 + \hat{\gamma}_2 + \hat{\gamma}_4 + \hat{\gamma}_7 + \hat{\gamma}_{11} + \hat{\delta}_1 + \hat{\delta}_2 + \hat{\delta}_4 + \hat{\delta}_7 = 3768 \qquad (143)$$

which is the second line of Equ. (145). Others may be written likewise, as shown in the following lines. In the twelfth line the corresponding statement is not made but rather there is, again, an appeal to the condition of Equ. (9), to avoid the algebraic problem of singularity, if Equ. (145) are treated as a set of simultaneous equations. For Columns the equations are:

$$\left.\begin{array}{l} 11\hat{\mu} + 11\hat{\beta}_1 = 806 \\ \cdots\cdots\cdots\cdots\cdots \\ 11\hat{\mu} + 11\hat{\beta}_4 = 771 \\ \hat{\beta}_1 + \hat{\beta}_2 + \hat{\beta}_3 + \hat{\beta}_4 + \hat{\beta}_5 = 0 \end{array}\right\} \qquad (144)$$

These are not included in Equ. (145) because it is somewhat bulky anyhow. Treatments and Carries-over are extensively and intimately confounded with each other and with Rows, as set forth in Equ. (145). In addition there is the peculiarity that each Carry-over is confounded with Column 1, although it is free of them! We have:

$\hat{\mu}$	$\hat{\alpha}_1$	$\hat{\alpha}_2$	$\hat{\alpha}_3$	$\hat{\alpha}_4$	$\hat{\alpha}_5$	$\hat{\alpha}_6$	$\hat{\alpha}_7$	$\hat{\alpha}_8$	$\hat{\alpha}_9$	$\hat{\alpha}_{10}$	$\hat{\alpha}_{11}$	$\hat{\beta}_1$	$\hat{\gamma}_1$	$\hat{\gamma}_2$	$\hat{\gamma}_3$	$\hat{\gamma}_4$	$\hat{\gamma}_5$	$\hat{\gamma}_6$	$\hat{\gamma}_7$	$\hat{\gamma}_8$	$\hat{\gamma}_9$	$\hat{\gamma}_{10}$	$\hat{\gamma}_{11}$	$\hat{\delta}_1$	$\hat{\delta}_2$	$\hat{\delta}_3$	$\hat{\delta}_4$	$\hat{\delta}_5$	$\hat{\delta}_6$	$\hat{\delta}_7$	$\hat{\delta}_8$	$\hat{\delta}_9$	$\hat{\delta}_{10}$	$\hat{\delta}_{11}$	=	Sum
55																																			=	3768
5	5												1	1		1			1				1	1	1		1			1					=	379
5		5											1	1	1		1			1					1	1		1			1				=	344
5			5											1	1	1		1			1					1	1		1			1			=	314
5				5											1	1	1		1			1					1	1		1			1		=	377
5					5											1	1	1		1			1					1	1		1			1	=	347
5						5							1				1	1	1		1			1					1	1		1			=	350
5							5							1				1	1	1		1			1					1	1		1		=	341
5								5							1				1	1	1		1			1					1	1		1	=	331
5									5				1			1				1	1	1		1			1					1	1		=	333
5										5				1			1				1	1	1		1			1					1	1	=	307
	1	1	1	1	1	1	1	1	1	1	1																								=	0
11												11																							=	806
5	1	1				1			1		1		5																		1	1	1	1	=	356
5	1	1	1				1			1				5										1								1	1	1	=	341
5		1	1	1				1			1				5									1	1								1	1	=	377
5	1		1	1	1				1							5								1	1	1								1	=	360
5		1		1	1	1				1							5							1	1	1	1								=	339
5			1		1	1	1				1							5							1	1	1	1							=	330
5	1			1		1	1	1											5							1	1	1	1						=	371
5		1			1		1	1	1											5							1	1	1	1					=	312
5			1			1		1	1	1											5							1	1	1	1				=	313
5				1			1		1	1	1											5							1	1	1	1			=	307
													1	1	1	1	1	1	1	1	1	1	1												=	0
4	1					1			1		1	-1		1	1	1	1							4											=	296
4	1	1					1			1		-1			1	1	1	1							4										=	267
4		1	1					1			1	-1				1	1	1	1							4									=	252
4	1		1	1					1			-1					1	1	1	1							4								=	267
4		1		1	1					1		-1						1	1	1	1							4							=	271
4			1		1	1					1	-1							1	1	1	1							4						=	267
4	1			1		1	1					-1								1	1	1	1							4					=	269
4		1			1		1	1				-1	1								1	1	1								4				=	260
4			1			1		1	1			-1	1	1								1	1									4			=	263
4				1			1		1	1		-1	1	1	1								1										4		=	292
																								1	1	1	1	1	1	1	1	1	1	1	=	0

(145)

(The positions left empty contain zeroes, not shown)

Let us concern ourselves first with the question of testing significance of the effects of direct Treatment, alone, i.e., without consideration of possible Carry-over. This will, of course, be as in the preceding section, in connection with Table XXVII. It may be summarized largely by saying that the variability residual on the control factors $\hat{\mu}$, Rows and Columns can be found by analysis of variance techniques, as discussed previously in connection with the latin square. The estimates are as in Equ. (142), for Rows as from the first 12 lines and columns of Equ. (145),

$$\begin{array}{ccccccccccc} \hat{\alpha}_1 & \hat{\alpha}_2 & \hat{\alpha}_3 & \hat{\alpha}_4 & \hat{\alpha}_5 & \hat{\alpha}_6 & \hat{\alpha}_7 & \hat{\alpha}_8 & \hat{\alpha}_9 & \hat{\alpha}_{10} & \hat{\alpha}_{11} \\ \hline +7.29 & +.29 & -5.71 & +6.89 & +.89 & +1.49 & -.31 & -2.31 & -1.91 & -7.11 & +.49 \end{array} \qquad (146)$$

and from Equ. (144),

$$\begin{array}{ccccc} \hat{\beta}_1 & \hat{\beta}_2 & \hat{\beta}_3 & \hat{\beta}_4 & \hat{\beta}_5 \\ \hline +4.76 & -5.51 & -3.42 & +1.58 & +2.58 \end{array} \; . \qquad (147)$$

The residual variability on estimates of $\hat{\mu}$, Rows and Columns, as control factors, is:

$$\begin{aligned} &261{,}576 - 68.51(3768) \\ &\quad - \{ +7.29(379) + .29(344) - \dots + .49(345)\} \\ &\quad - \{ + 4.76(806) - 5.51(693) - \dots + 2.58(782)\} \\ &\quad\quad = 1639.96 \end{aligned} \qquad (148)$$

as shown in Table XXVIIIb, if the thing be carried out with high accuracy, of the estimates. Then the situation increased by the consideration of the direct effect of Treatment will require consideration of Equ. (145) with the columns (25th through 35th) and the corresponding lines eliminated, i.e., those involving the Carries-over. Thus we have the equations,

$$\begin{array}{ccccccccccccccccccccccccc}
\hat{\mu} & \hat{\alpha}_1 & \hat{\alpha}_2 & \hat{\alpha}_3 & \hat{\alpha}_4 & \hat{\alpha}_5 & \hat{\alpha}_6 & \hat{\alpha}_7 & \hat{\alpha}_8 & \hat{\alpha}_9 & \hat{\alpha}_{10} & \hat{\alpha}_{11} & \hat{\gamma}_1 & \hat{\gamma}_2 & \hat{\gamma}_3 & \hat{\gamma}_4 & \hat{\gamma}_5 & \hat{\gamma}_6 & \hat{\gamma}_7 & \hat{\gamma}_8 & \hat{\gamma}_9 & \hat{\gamma}_{10} & \hat{\gamma}_{11} & = & \text{Sum} \\
\hline
55 & & & & & & & & & & & & & & & & & & & & & & & = & 3768 \\
5 & 5 & & & & & & & & & & & 1 & 1 & & 1 & & & 1 & & & & 1 & = & 379 \\
5 & & 5 & & & & & & & & & & 1 & 1 & 1 & & 1 & & & 1 & & & & = & 344 \\
5 & & & 5 & & & & & & & & & & 1 & 1 & 1 & & 1 & & & 1 & & & = & 314 \\
5 & & & & 5 & & & & & & & & & & 1 & 1 & 1 & & 1 & & & 1 & & = & 377 \\
5 & & & & & 5 & & & & & & & & & & 1 & 1 & 1 & & 1 & & & 1 & = & 347 \\
5 & & & & & & 5 & & & & & & 1 & & & & 1 & 1 & 1 & & 1 & & & = & 350 \\
5 & & & & & & & 5 & & & & & & 1 & & & & 1 & 1 & 1 & & 1 & & = & 341 \\
5 & & & & & & & & 5 & & & & & & 1 & & & & 1 & 1 & 1 & & 1 & = & 331 \\
5 & & & & & & & & & 5 & & & 1 & & & 1 & & & & 1 & 1 & 1 & & = & 333 \\
5 & & & & & & & & & & 5 & & & 1 & & & 1 & & & & 1 & 1 & 1 & = & 307 \\
 & 1 & 1 & 1 & 1 & 1 & 1 & 1 & 1 & 1 & 1 & 1 & & & & & & & & & & & & = & 0 \\
5 & 1 & 1 & & & & 1 & & & 1 & & 1 & 5 & & & & & & & & & & & = & 356 \\
5 & 1 & 1 & 1 & & & & 1 & & & 1 & & & 5 & & & & & & & & & & = & 341 \\
5 & & 1 & 1 & 1 & & & & 1 & & & 1 & & & 5 & & & & & & & & & = & 377 \\
5 & 1 & & 1 & 1 & 1 & & & & 1 & & & & & & 5 & & & & & & & & = & 360 \\
5 & & 1 & & 1 & 1 & 1 & & & & 1 & & & & & & 5 & & & & & & & = & 339 \\
5 & & & 1 & & 1 & 1 & 1 & & & & 1 & & & & & & 5 & & & & & & = & 330 \\
5 & 1 & & & 1 & & 1 & 1 & 1 & & & & & & & & & & 5 & & & & & = & 371 \\
5 & & 1 & & & 1 & & 1 & 1 & 1 & & & & & & & & & & 5 & & & & = & 312 \\
5 & & & 1 & & & 1 & & 1 & 1 & 1 & & & & & & & & & & 5 & & & = & 313 \\
5 & & & & 1 & & & 1 & & 1 & 1 & 1 & & & & & & & & & & 5 & & = & 307 \\
 & & & & & & & & & & & & 1 & 1 & 1 & 1 & 1 & 1 & 1 & 1 & 1 & 1 & 1 & = & 0
\end{array} \qquad (149)$$

(The positions left empty contain zeroes, not shown)

from which the estimates for Rows are:

$\hat{\alpha}_1$	$\hat{\alpha}_2$	$\hat{\alpha}_3$	$\hat{\alpha}_4$	$\hat{\alpha}_5$	$\hat{\alpha}_5$	$\hat{\alpha}_7$	$\hat{\alpha}_8$	$\hat{\alpha}_9$	$\hat{\alpha}_{10}$	$\hat{\alpha}_{11}$
+4.77	-.23	-6.86	+5.95	+1.45	+1.86	+2.00	-3.64	+.77	-5.77	-.32

(150)

the estimates for Treatment are as shown in Table XXVIIIb, while, of course, the estimates for Columns remain as in Equ. (147). The residual variability now becomes

$$\begin{aligned}
&261{,}576 - 68.51(3768) \\
&\qquad - \{ + 4.77(379) - .23(344) - \ldots - .32(345)\} \\
&\qquad - \{ + 4.76(806) - 5.51(693) - \ldots + 2.58(782)\} \\
&\qquad - \{ + 1.32(356) + .91(341) + \ldots + 4.59(362)\} \\
&\qquad\qquad = 686.20
\end{aligned} \qquad (151)$$

if the thing be carried out with higher accuracy, of the estimates. These results are summarized in Table XXVIIIb; the value F is highly significant.

The practical way of going about the analysis of a situation where there are Rows, Columns, with possible treatment effect and a possible carry-over effect is fundamentally the same as for latin squares, previously. It is as follows. Find the residual sum of squares on the Rows, Columns and Carry-over and then find the residual when direct Treatment is included. The question is then whether the reduction in residual squares due to the experimental Treatment, over the control factors, is significant. Following previous procedure, the columns involving $\hat{\gamma}_k$ and corresponding rows of Equ. (145) must be dropped, along the lines of setting up Equ. (55) from (48). We get :

$$\left.\begin{array}{ccccccccccccccccccccccccccr}
\hat{\mu} & \hat{\alpha}_1 & \hat{\alpha}_2 & \hat{\alpha}_3 & \hat{\alpha}_4 & \hat{\alpha}_5 & \hat{\alpha}_6 & \hat{\alpha}_7 & \hat{\alpha}_8 & \hat{\alpha}_9 & \hat{\alpha}_{10} & \hat{\alpha}_{11} & \hat{\beta}_1 & \hat{\delta}_1 & \hat{\delta}_2 & \hat{\delta}_3 & \hat{\delta}_4 & \hat{\delta}_5 & \hat{\delta}_6 & \hat{\delta}_7 & \hat{\delta}_8 & \hat{\delta}_9 & \hat{\delta}_{10} & \hat{\delta}_{11} & = & \text{Sum} \\
\hline
55 & &&&&&&&&&& & & &&&&&&&&&& & = & 3768 \\
5 & 5&&&&&&&&&& & & 1&1&&1&&&1&&&& & = & 379 \\
5 & &5&&&&&&&&& & & &1&1&&1&&&1&&& & = & 344 \\
5 & &&5&&&&&&&& & & &&1&1&&1&&&1&& & = & 314 \\
5 & &&&5&&&&&&& & & &&&1&1&&1&&&1& & = & 377 \\
5 & &&&&5&&&&&& & & &&&&1&1&&1&&&1 & = & 347 \\
5 & &&&&&5&&&&& & & 1&&&&&1&1&&1&& & = & 350 \\
5 & &&&&&&5&&&& & & &1&&&&&1&1&&1& & = & 341 \\
5 & &&&&&&&5&&& & & &&1&&&&&1&1&&1 & = & 331 \\
5 & &&&&&&&&5&& & & 1&&&1&&&&&1&1& & = & 333 \\
5 & &&&&&&&&&5& & & &1&&&1&&&&&1&1 & = & 307 \\
 & 1&1&1&1&1&1&1&1&1&1&1 & & &&&&&&&&&& & = & 0 \\
11 & &&&&&&&&&& & 11 & &&&&&&&&&& & = & 806 \\
4 & 1&&&&&1&&&1&&1 & -1 & 4&&&&&&&&&& & = & 296 \\
4 & 1&1&&&&&1&&&1& & -1 & &4&&&&&&&&& & = & 267 \\
4 & &1&1&&&&&1&&&1 & -1 & &&4&&&&&&&& & = & 252 \\
4 & 1&&1&1&&&&&1&& & -1 & &&&4&&&&&&& & = & 267 \\
4 & &1&&1&1&&&&&1& & -1 & &&&&4&&&&&& & = & 271 \\
4 & &&1&&1&1&&&&&1 & -1 & &&&&&4&&&&& & = & 267 \\
4 & 1&&&1&&1&1&&&& & -1 & &&&&&&4&&&& & = & 269 \\
4 & &1&&&1&&1&1&&& & -1 & &&&&&&&4&&& & = & 260 \\
4 & &&1&&&1&&1&1&& & -1 & &&&&&&&&4&& & = & 263 \\
4 & &&&1&&&1&&1&1& & -1 & &&&&&&&&&4& & = & 292 \\
 & &&&&&&&&&& & & 1&1&1&1&1&1&1&1&1&1&1 & = & 0
\end{array}\right\}(152)$$

(The positions left empty contain zeroes, not shown)

We shall again be interested in the variability residual on the control factors, when Carry-over is regarded as a control factor. Solving Equ. (152) and Equ. (144), the results are:

$\hat{\alpha}_1$	$\hat{\alpha}_2$	$\hat{\alpha}_3$	$\hat{\alpha}_4$	$\hat{\alpha}_5$	$\hat{\alpha}_6$	$\hat{\alpha}_7$	$\hat{\alpha}_8$	$\hat{\alpha}_9$	$\hat{\alpha}_{10}$	$\hat{\alpha}_{11}$
+7.43	+1.66	-4.80	+6.55	+1.67	+1.01	-.30	-1.02	-4.25	-8.22	+.27

$\hat{\beta}_1$	$\hat{\beta}_2$	$\hat{\beta}_3$	$\hat{\beta}_4$	$\hat{\beta}_5$
+4.76	-5.51	-3.42	+1.58	+2.58

$\hat{\delta}_1[\hat{1}]$	$\hat{\delta}_2[\hat{2}]$	$\hat{\delta}_3[\hat{3}]$	$\hat{\delta}_4[\hat{4}]$	$\hat{\delta}_5[\hat{5}]$	$\hat{\delta}_6[\hat{6}]$	$\hat{\delta}_7[\hat{7}]$	$\hat{\delta}_8[\hat{8}]$	$\hat{\delta}_9[\hat{9}]$	$\hat{\delta}_{10}[\widehat{10}]$	$\hat{\delta}_{11}[\widehat{11}]$
+5.57	-.71	-3.35	-1.80	+.02	-.10	-3.74	-2.82	+.70	+7.24	-.99

(153)

Finally it is necessary to multiply the estimates of Equ. (153) by the appropriate right-hand members of Equ. (144) and (145) to produce the residual variability on control factors in Table XXVIIIc. Thus there may be calculated

$$\begin{aligned} &261{,}576 - 68.51(3768) \\ &\quad - \{+7.43(379) + 1.66(344) + \ldots + .27(345)\} \\ &\quad - \{+4.76(806) - 5.51(693) + \ldots + 2.58(782)\} \\ &\quad - \{+5.57(296) - .71(267) + \ldots - .99(258)\} \\ &\qquad = 1212.01 \end{aligned} \qquad (154)$$

or if estimates with more figures are used the result is 1213.02, as shown in Table XXVIIIc. It is now necessary to find the estimates of effects and residual squares on the full model with $\hat{\mu}$, Rows, Columns and Carries-over, as before, but also with Treatments. Equ. (144) remains undisturbed but Equ. (145) have to be employed in full. Solving, the estimates for Row effects become

$\hat{\alpha}_1$	$\hat{\alpha}_2$	$\hat{\alpha}_3$	$\hat{\alpha}_4$	$\hat{\alpha}_5$	$\hat{\alpha}_6$	$\hat{\alpha}_7$	$\hat{\alpha}_8$	$\hat{\alpha}_9$	$\hat{\alpha}_{10}$	$\hat{\alpha}_{11}$
+4.05	+.77	-10.56	+2.40	-.21	-1.63	+4.63	+1.04	+1.15	-1.82	+.19

(155)

while the estimates for $\hat{\gamma}_k$ or $(\hat{k})$ and $\hat{\delta}_\ell$ or $[\hat{\ell}]$ are as shown in Table XXVIIIc. The residual variability becomes

$$
\begin{aligned}
&261{,}576 - 68.51(3768) \\
&\quad - \{ + 4.05(379) + .77(344) - \ldots + .19(345)\} \\
&\quad - \{ + 4.76(806) - 5.51(693) + \ldots + 2.58(^{-}32)\} \\
&\quad - \{ + 3.44(356) + 1.86(341) + \ldots + 2.15(362)\} \\
&\quad - \{ - .19(296) - 8.00(267) - \ldots - 9.02(258)\} \\
&\qquad = 250.86 \qquad (156)
\end{aligned}
$$

if the thing is carried out with higher accuracy, of the estimates. These results are summarized in Table XXVIIIc. The value of F has become even more highly significant with the introduction of Carry-over into the model.

To handle a Youden square, with possible Carry-over, the least squares equations must be solved on electronic computer--this is necessary equipment. It is impractical to solve the equations with a desk computer. Perhaps that is why the full and proper analysis of Carry-over did not appear historically sooner--there was hardly equipment to solve the necessary equations. It will be readily realized that the set of least squares equations may become intolerably large, particularly if there are a great many Rows, as there often are in practice. This difficulty can be avoided by certain procedures discussed in the Appendix on electronic programs, so that the size of the set of equations, to be dealt with, depends only on t and c . As was discussed previously in connection with latin squares, onethere simply gives the machine the Design and the data so that it can set itself up the equations to solve, to calculate the various residual variabilities and the values of F .

The significance of Carry-over, when allowance is made for all the other factors, including direct Treatment, as control factors may be made. It follows the same line as above when the reduction in sum of squares directly attributable to the Treatments and its significance were considered. The matter is simply one of the order in which one forms the two lines as in Table XXVIIIc, i.e., the order in which one takes out the effects. Probably in a general practical way, it will not matter whether or no Carry-over is significant; it can be taken out anyhow just as we normally take out the effects of Rows and Columns regardless of their significance. On the other hand, if one wanted to test for the significance of Carry-over, per se, one would find the residual sum of squares on the Rows, Columns and Treatment and then find the Residual when all four factors are included. The question is then whether the reduction due to the introduction of Carry-over is significant. From a calculating point of view the matter is very simple, as can be seen from Table XXVIII. The variability residual on $\hat{\mu}$, Rows, Columns and Treatments (control) has been previously calculated, as in XXVIIIb, in connection with the test without Carry-over. The variability residual on the entire model has just been calculated in the test for Treatments with Carry-over in the model, as in Table XXVIIIc. In the present case Carry-over with $F = 3.47$ is significant at the 5% level.

Table XXXIII - Youden rectangle, 11x5x11, illustrating Carry-over

a. Satisfaction by Groups, Weeks and Treatments

			Week			
Group	1	2	3	4	5	Sum
I	(1) 82	(2) 68	(4) 70	(7) 78	(11) 81	379
II	(2) 74	(3) 65	(5) 65	(8) 65	(1) 75	344
III	(3) 74	(4) 57	(6) 54	(9) 60	(2) 69	314
IV	(4) 82	(5) 70	(7) 72	(10) 71	(3) 82	377
V	(5) 76	(6) 68	(8) 64	(11) 68	(4) 71	347
VI	(6) 72	(7) 76	(9) 60	(1) 74	(5) 68	350
VII	(7) 79	(8) 57	(10) 58	(2) 75	(6) 72	341
VIII	(8) 61	(9) 59	(11) 69	(3) 76	(7) 66	331
IX	(9) 68	(10) 51	(1) 69	(4) 80	(8) 65	333
X	(10) 60	(11) 66	(2) 55	(5) 60	(9) 66	307
XI	(11) 78	(1) 56	(3) 80	(6) 64	(10) 67	345
Sum	806	693	716	771	782	3768
Mean	73.3	63.0	65.1	70.1	71.1	68.5
Sum Sq.	59,630	44,221	47,232	54,547	55,946	

b. Analysis for direct Treatment alone

	$\widehat{(1)}$	$\widehat{(2)}$	$\widehat{(3)}$	$\widehat{(4)}$	$\widehat{(5)}$	$\widehat{(6)}$	$\widehat{(7)}$	$\widehat{(8)}$	$\widehat{(9)}$	$\widehat{(10)}$	$\widehat{(11)}$
Contr.	+1.32	+.91	+7.91	+2.27	-1.36	-2.14	+3.50	-6.18	-3.18	-7.64	+4.59
Adj.Mn.	69.83	69.42	76.42	70.78	67.15	66.37	72.01	62.33	65.33	60.87	73.10

Factors	d.f.	Residual Variability (Squares)
$\hat{\mu}$, Rows & Columns (control)	40	1639.96
Control factors plus Treatments	30	686.20

$$F_{10,30} = \frac{(1639.96 - 686.20)/10}{686.20/30} = 4.17^{**} \quad .$$

c. Direct Treatment and Carry-over estimates from simultaneous equations

	$(\hat{1})$	$(\hat{2})$	$(\hat{3})$	$(\hat{4})$	$(\hat{5})$	$(\hat{6})$	$(\hat{7})$	$(\hat{8})$	$(\hat{9})$	$(\widehat{10})$	$(\widehat{11})$
Contr.	+3.44	+1.86	+11.70	+8.58	+1.57	+.23	+1.25	-11.89	-7.27	-11.62	+2.15
Adj. Mn.	71.95	70.37	80.21	77.09	70.08	68.74	69.76	56.62	61.24	56.89	70.66

	$[\hat{1}]$	$[\hat{2}]$	$[\hat{3}]$	$[\hat{4}]$	$[\hat{5}]$	$[\hat{6}]$	$[\hat{7}]$	$[\hat{8}]$	$[\hat{9}]$	$[\widehat{10}]$	$[\widehat{11}]$
Contr.	-.19	-8.00	-5.09	+2.39	+4.57	+9.87	+4.73	-.55	+1.98	-.70	-9.02

d. Significance of direct treatment effects

Factors	d.f.	Residual Variability (Squares)
$\hat{\mu}$, Rows, Columns &Carries-over (control)	30	1213.02
Control factors plus Treatments	20	250.86

$$F_{10,20} = \frac{(1213.02 - 250.86)/10}{250.86/20} = 7.67^{***}$$

e. Significance of carry-over effects

Factors	d.f.	Residual Variability (Squares)
$\hat{\mu}$, Rows, Columns & Treatments (control)	30	686.20
Control factors plus Carries-over	20	250.86

$$F_{10,20} = \frac{(686.20 - 250.86)/10}{250.86/20} = 3.47^{*}$$

f. Satisfaction by Treatment and by previous Treatment

A crude analysis

	Treatment														
fter	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	Sum	Mean	Adj. Mean	Contr.
(1)		68	80	80	68							296	74.0	69.7	+2.4
(2)			65	70	60	72						267	66.8	62.8	-4.5
(3)				57	65	64	66					252	63.0	59.9	-7.4
(4)					70	54	78	65				267	66.8	67.7	+.4
(5)						68	72	65	66			271	67.8	71.2	+3.9
(6)							76	64	60	67		267	66.8	72.3	+5.0
(7)								57	60	71	81	269	67.2	73.6	+6.3
(8)	75								59	58	68	260	65.0	68.0	+.7
(9)	74	69								51	69	263	65.8	66.4	-.9
10)	69	75	82								66	292	73.0	69.2	+1.9
11)	56	55	76	71								258	64.5	59.5	-7.8
k. gd.	82	74	74	82	76	72	79	61	68	60	78	806			
um	356	341	377	360	339	330	371	312	313	307	362	3768			
ean	71.2	68.2	75.4	72.0	67.8	66.0	74.2	62.4	62.6	61.4	72.4			68.5	
dj.Mn.	72.4	69.1	77.0	75.5	69.6	67.5	73.8	59.3	59.4	59.2	70.8				
ontr.	+3.9	+.6	+8.5	+7.0	+1.1	-1.0	+5.3	-9.2	-9.1	-9.3	+2.3				

To the other calculations there has been added Table XXVIIIb which is a simple calculation of the form previously recommended in connection with latin squares, under certain circumstances. It is not very suitable here first because the iterative calculation is most extensive, as is discussed in Chap. IX. Further, it can be seen from the comparison of the contributions of Treatment and Carry-over with the truth of XXVIIIc that the estimates are very bad. This is understandable because not only are the two things badly confounded, not only are they heavily confounded with Row effects but the effect of Treatment and Carry-over are pretty well confounded with the same row effects so that there must be a spurious positive correlation between them.

The estimates for Treatment and Carry-over, as in Table XXVIIIc, show, obviously, little correspondence. Their correlation coefficient is -.14.

As was said much earlier, in Chap. V, on the general problem of analysis, to the writer it seems generally of little interest to test the significance of Rows or Columns. Nonetheless, circumstances govern cases and such may be useful. Accordingly, for this one problem such analysis will be made on the same general lines as for Treatments or Carries-over. This may be done conveniently in the model for direct Treatment above, i.e., without Carry-over, as in Table XXXIII b. The total equations are as in Equ. (149). To test for Rows it is necessary to drop from Equ. (149) the lines involving $\hat{\alpha}_1$. When this simple set of equations is solved, together with the estimates of $\hat{\beta}_j$ from Equ. [illegible]53), and the resultant estimates applied to the right-hand quantities of the equations, the sum of residual squares is found. An operation such as that of Equ. (156) is involved. This quantity may be subtracted from the total variability as in Table XXXIIIb. The results are as follows:

Factors	d.f.	Residual Variability (Squares)
$\hat{\mu}$, Columns & Treatments (control)	40	1400.28
Control factors plus Rows	30	686.20

$$F_{10,30} = \frac{(1400.28 - 686.20)/10}{686.20/30} = 3.12 ^{**}$$

Again, if one wished to test for the effect of Columns, one would solve Equ. (149), making no allowance for the estimates of $\hat{\beta}$, and make a calculation of residual variability like Equ. (156). The result would be

Factors	d.f.	Residual Variability (Squares)
$\hat{\mu}$, Rows & Treatments (control)	34	1499.40
Control factors plus Columns	30	686.20

$$F_{4,30} = \frac{(1499.40 - 686.20)/4}{686.20/30} = 8.89 \text{ ***}$$

The attempt to extend such method to the model with Carry-over, as in Table XXXIII d or e will encounter algebraic difficulty, because of the confounding of β_1 with Carry-over.

Explicit solution for simple cases of Carry-over - In connection with latin squares, it was suggested that explicit solution for the effect of Treatments and Carries-over, in terms of sums of observations, was not practical, and that it is best to solve directly from least squares equations. It was, however, pointed out that the consideration of possible explicit solutions was useful in discovering whether a Design would yield certain estimates. There is a risk it will not when t is small. Actually, there is little temptation for men with algebraic ability to attempt an explicit solution of equations such as (145).

The smallest Youden rectangle, $c < t$, is the Yates Design 3x2x3 which is discussed in Chap. VIII since it is also a paired test. The next Yates Design is 4x3x4, i.e.:

	Period		
Group	1	2	3
I	(1) y_{111}	(3) y_{1231}	(4) y_{1343}
II	(2) y_{212}	(4) y_{2242}	(1) y_{2314}
III	(3) y_{313}	(1) y_{3213}	(2) y_{3321}
IV	(4) y_{414}	(2) y_{4224}	(3) y_{4332}

It is possible to solve for Row, Column, Treatment and Carry-over effects since there are 12 parameters:

$\hat{\mu}$	1		
Groups	3	Treatments	3
Periods	2	Carries-over	3

to be discovered from 12 observations. The situation is, indeed, exactly determined. There is no over-determination available for the estimation of extraneous variability and such is the essence of statistics. The Design can, of course, be repeated, say once, and then there become available 8 degrees of freedom for the estimation of extraneous variability. Obviously, for all Yates rectangles, $t \times (t - 1) \times t$, $t > 4$, it must be possible to distinguish the Carry-over from the Treatments proper. Repetition of the Design may be necessary to get an adequate number of degrees of freedom.

Apart from the Yates rectangles, in the smallest Youden Design 7x3x7, i.e.:

	Period		
Group	1	2	3
I	(1) y_{111}	(2) y_{1221}	(4) y_{1342}
II	(2) y_{212}	(3) y_{2232}	(5) y_{2353}
III	(3) y_{313}	(4) y_{3243}	(6) y_{3364}
IV	(4) y_{414}	(5) y_{4254}	(7) y_{4375}
V	(5) y_{515}	(6) y_{5265}	(1) y_{5316}
VI	(6) y_{616}	(7) y_{6276}	(2) y_{6327}
VII	(7) y_{717}	(1) y_{7217}	(3) y_{7331}

it is possible to solve for row, column, treatment, and carry-over effects since there are 21 parameters to be found from 21 observations. The solution is again mathematical rather than a statistical matter of true least squares estimation. In all the higher Youden rectangles there are enough observations in the pattern to make it possible to estimate Carry-over independent of Treatment, proper, and vice versa. Repetition of the Design may be necessary to get an adequate number of degrees of freedom.

Youden rectangles with missing Rows - As was previously observed in connection with latin squares, under the practical conditions of experimentation it is very common, indeed almost usual, for there to be some data missing. It is very common, indeed almost usual, for there to be some data missing from a Youden Design of almost any type. Entire Rows missing, is much more common than single observations missing. Accordingly, it becomes not a matter of dealing with a table where here and there an observation is missing but of dealing with a Design where 2 of the 19 Rows are missing and so the balance of the experiment is much disturbed. The practical situation is actually worse than is commonly pictured in the literature, when an observation is supposed lost here and there. Under practical conditions, as was discussed previously in connection with the latin square, one is not only disappointed in a Machine's or a Man's not completing his assignment but in fact of, say, 19 Machines, assigned, 2 do not appear at all; they are withdrawn for other purposes. One man fails to complete assignment of Changes-over. The effect, for a Youden rectangle, is much more serious than for a latin square where a whole Row is missing, because there every Row contains every Treatment. In the Youden rectangle as soon as a Row is lost, the balance of Treatments is upset.

A situation with two arbitrarily chosen Rows missing is shown in Table XXIX, derived from Table XXVIII. Even if there be only interest in the simple model with direct treatment effect and no Carry-over, it seems hardly practical to do anything but use the least squares equations or a program in an electric computer involving them. If we write out the equations a great many will be modified by the incompleteness of the Design. In the program shown in the Appendix, note that there need be given no indication of how the data are incomplete--indeed, the Rows may be renumbered. Regardless of method, the results are as shown in Table XXIXc.

The disturbance in the situation may be appreciated by considering briefly the least squares equations for Table XXIX. Let us restrict ourselves, for the moment, to the model with only direct Treatment effects, i.e., without Carry-over. Now form the estimate of $\hat{\mu}$ from the sum of all observations, i.e.,

$$\begin{aligned} &45\hat{\mu} + 4(\hat{\alpha}_1 + \hat{\alpha}_2 + \hat{\alpha}_3 + \hat{\alpha}_4 + \hat{\alpha}_5 + \hat{\alpha}_6 + \hat{\alpha}_7 + \hat{\alpha}_8 + \hat{\alpha}_9) + 9(\hat{\beta}_1 + \hat{\beta}_2 + \hat{\beta}_3 + \hat{\beta}_4) \\ &\quad + 4\hat{\gamma}_1 + 4\hat{\gamma}_2 + 4\hat{\gamma}_3 + 5\hat{\gamma}_4 + 5\hat{\gamma}_5 + 3\hat{\gamma}_6 + 4\hat{\gamma}_7 + 4\hat{\gamma}_8 + 5\hat{\gamma}_9 + 3\hat{\gamma}_{10} + 4\hat{\gamma}_{11} \\ &\quad = 3768 \end{aligned} \tag{157}$$

but taking advantage of the zero-centering of results, i.e., Equ. (9) and (135)

$$45\hat{\mu} + \hat{\gamma}_4 + \hat{\gamma}_5 - \hat{\gamma}_6 + \hat{\gamma}_9 - \hat{\gamma}_{10} = 3768 \tag{158}$$

the moral of which is that one's estimate of the general level depends on what Treatments occur frequently and what occur infrequently. The practical thought is that the equations are going to be a lot more difficult than in the perfect and complete Youden rectangles.

Not only is $\hat{\mu}$ confounded in some measure with treatment effects but so are the quantities $\hat{\beta}_j$, which accordingly became involved in the general system of simultaneous equations--they no longer come from their own independent set of equations as previously in Equ. (144). All the above is true when only direct treatment effects are involved. When Carry-over is also admitted it much contributes to the confounding and complexity. The total situation is shown in matrix form in Equ. (159) which may be compared with Equ. (145). The situation tends to be intolerable for direct handling; it should be understood,but it is best left if possible to the electronic computer as in the appendix program. The full matrix is:

1	$\hat{\alpha}_2$	$\hat{\alpha}_3$	$\hat{\alpha}_4$	$\hat{\alpha}_5$	$\hat{\alpha}_6$	$\hat{\alpha}_7$	$\hat{\alpha}_8$	$\hat{\alpha}_9$	$\hat{\beta}_1$	$\hat{\beta}_2$	$\hat{\beta}_3$	$\hat{\beta}_4$	$\hat{\beta}_5$	$\hat{\gamma}_1$	$\hat{\gamma}_2$	$\hat{\gamma}_3$	$\hat{\gamma}_4$	$\hat{\gamma}_5$	$\hat{\gamma}_6$	$\hat{\gamma}_7$	$\hat{\gamma}_8$	$\hat{\gamma}_9$	$\hat{\gamma}_{10}$	$\hat{\gamma}_{11}$	$\hat{\delta}_1$	$\hat{\delta}_2$	$\hat{\delta}_3$	$\hat{\delta}_4$	$\hat{\delta}_5$	$\hat{\delta}_6$	$\hat{\delta}_7$	$\hat{\delta}_8$	$\hat{\delta}_9$	$\hat{\delta}_{10}$	$\hat{\delta}_{11}$	= Sum
																	1	1	-1			1	-1					1	1				1			= 3082
5														1	1		1			1				1	1	1		1			1					= 379
	5													1	1	1		1			1					1	1		1			1				= 344
		5													1	1	1		1			1					1	1		1			1			= 314
			5													1	1	1		1			1					1	1		1			1		= 377
				5													1	1	1		1			1					1	1		1			1	= 347
					5									1				1	1	1		1			1					1	1		1			= 350
						5										1				1	1	1		1			1					1	1		1	= 331
							5							1			1				1	1	1		1			1					1	1		= 333
1	1	1	1	1	1	1	1	1																												= 0
									9											-1				-1												= 649
										9				-1							-1										-1				-1	= 580
											9					-1							-1		-1							-1				= 578
												9			-1				-1								-1							-1		= 632
									1	1	1	1	1																							= 0
1	1				1		1			-1				4																		1	1	1		= 300
1	1	1						1				-1			4										1								1		1	= 266
	1	1	1			1					-1					4										1								1	1	= 297
1		1	1	1			1										5								1	1	1								1	= 360
	1		1	1	1			1										5							1	1	1	1								= 339
		1		1	1							-1	-1						3									1	1							= 194
1			1		1	1			-1											4							1	1	1	1						= 292
	1			1		1	1			-1											4							1	1	1						= 255
		1			1	1	1	1														5							1	1	1	1				= 313
			1				1	1			-1		-1										3								1		1			= 182
														1	1	1	1	1	1	1	1	1	1	1												= 0
1					1		1		-1		-1				1		1	1							3											= 216
1	1							1	-1				-1			1	1	1								3										= 195
	1	1				1			-1			-1					1	1		1							3									= 188
1		1	1				1		-1									1	1	1	1							4								= 267
	1		1	1				1	-1										1	1	1	1							4							= 271
		1		1	1				-1				-1							1	1	1								3						= 200
1			1		1				-1	-1												1	1	1							3					= 212
	1			1		1			-1		-1			1								1		1								3				= 202
		1			1	1	1		-1					1	1								1	1									4			= 263
			1				1	1	-1			-1		1		1								1										3		= 217
																									1	1	1	1	1	1	1	1	1	1	1	= 0

(159)

(The positions left empty contain zeroes, not shown)

In order to make the analysis, as in Table XXIXb for the model with Treatment effects only, it is first necessary, as usual, to find the variability residual on the control factors $\hat{\mu}$, Row and Column. This is very easily done, since they are orthogonal. It may be done by analysis of variance techniques or by neglecting the elements of Treatment and Carry-over, i.e., the columns (18th through 39th), involving $\hat{\gamma}_k$ and $\hat{\delta}_\ell$ of Equ. (159) and the corresponding rows. In the latter case the resultant estimates are:

$$\left.\begin{array}{c} \hat{\mu} = 68.49 \\ \begin{array}{ccccccccc} \hat{\alpha}_1 & \hat{\alpha}_2 & \hat{\alpha}_3 & \hat{\alpha}_4 & \hat{\alpha}_5 & \hat{\alpha}_6 & \hat{\alpha}_7 & \hat{\alpha}_8 & \hat{\alpha}_9 \\ \hline +7.31 & +.31 & -5.69 & +6.91 & +.91 & +1.51 & -2.29 & -1.89 & -7.09 \end{array} \\ \\ \begin{array}{ccccc} \hat{\beta}_1 & \hat{\beta}_2 & \hat{\beta}_3 & \hat{\beta}_4 & \hat{\beta}_5 \\ \hline +3.62 & -4.04 & -4.27 & +1.73 & +2.96 \end{array} \end{array}\right\} \qquad (160)$$

The residual is found, as usual, by taking from 213,708 the sum of products of these estimates times the appropriate right-hand constants of Equ.(159) to give 1111.20 as shown in Table XXIXa. In order to find the further reduction due to Treatments it is necessary to consider Equ. (159) less the 29th through 39th columns and the corresponding rows. The resultant estimates are:

$$\left.\begin{array}{c} \hat{\mu} = 68.31 \\ \begin{array}{ccccccccc} \hat{\alpha}_1 & \hat{\alpha}_2 & \hat{\alpha}_3 & \hat{\alpha}_4 & \hat{\alpha}_5 & \hat{\alpha}_6 & \hat{\alpha}_7 & \hat{\alpha}_8 & \hat{\alpha}_9 \\ \hline +4.55 & -.20 & -6.27 & +6.99 & +1.29 & +1.32 & -3.10 & +.54 & -5.11 \end{array} \\ \begin{array}{ccccc} \hat{\beta}_1 & \hat{\beta}_2 & \hat{\beta}_3 & \hat{\beta}_4 & \hat{\beta}_5 \\ \hline +4.68 & -3.98 & -4.51 & +1.83 & +1.98 \end{array} \end{array}\right\} \qquad (161)$$

and estimates of $\hat{\gamma}_k$ or $(\hat{k})$, as shown in Table XXIXb. The residual is found, as usual, by taking from 213,708 the sum of products of these estimates times the appropriate right-hand constants of Equ. (159) to give 454.76. These results are summarized in Table XXIXb.

In order to test the significance of Treatments, as in Table XXIXc, for the model with allowance for Carry-over, it is, of course, first necessary to find the variability residual on the control factors, $\hat{\mu}$, Row, Column and Carry-over; it is necessary to use Equ. (159) with Treatments dropped. This, probably, by setting all $\hat{\gamma}_k$ at zero. The resultant estimates are:

$$\left.\begin{array}{c}
\hat{\mu} = 68.54 \\
\begin{array}{ccccccccc}
\hat{\alpha}_1 & \hat{\alpha}_2 & \hat{\alpha}_3 & \hat{\alpha}_4 & \hat{\alpha}_5 & \hat{\alpha}_6 & \hat{\alpha}_7 & \hat{\alpha}_8 & \hat{\alpha}_9 \\
\hline
+8.82 & +1.25 & -5.09 & +6.65 & +.45 & +1.82 & -2.14 & -3.06 & -8.70
\end{array} \\
\begin{array}{ccccc}
\hat{\beta}_1 & \hat{\beta}_2 & \hat{\beta}_3 & \hat{\beta}_4 & \hat{\beta}_5 \\
\hline
+3.57 & -4.30 & -4.39 & +2.30 & +2.83
\end{array} \\
\begin{array}{ccccccccccc}
\hat{\delta}_1 & \hat{\delta}_2 & \hat{\delta}_3 & \hat{\delta}_4 & \hat{\delta}_5 & \hat{\delta}_6 & \hat{\delta}_7 & \hat{\delta}_8 & \hat{\delta}_9 & \hat{\delta}_{10} & \hat{\delta}_{11} \\
\hline
+.66 & -1.86 & -1.92 & -2.73 & +.19 & +1.20 & -3.88 & -1.33 & +.22 & +7.45 & +2.01
\end{array}
\end{array}\right\} \quad (162)$$

The residual is found, as usual, by taking from 213,708 the sum of products of these estimates times the appropriate right-hand constants of Equ. (159) to give 867.69. In order to find the further reduction due to Treatments it is necessary to consider the full model, with all factors, of Equ. (159). The resultant estimates are:

$$\left.\begin{array}{c} \hat{\mu} = 67.88 \\ \begin{array}{ccccccccc} \hat{\alpha}_1 & \hat{\alpha}_2 & \hat{\alpha}_3 & \hat{\alpha}_4 & \hat{\alpha}_5 & \hat{\alpha}_6 & \hat{\alpha}_7 & \hat{\alpha}_8 & \hat{\alpha}_9 \\ \hline +5.51 & +1.19 & -10.16 & +1.40 & -.51 & -1.33 & +2.27 & +2.13 & -.51 \end{array} \\ \begin{array}{ccccc} \hat{\beta}_1 & \hat{\beta}_2 & \hat{\beta}_3 & \hat{\beta}_4 & \hat{\beta}_5 \\ \hline +4.31 & -4.44 & -4.06 & +2.05 & +2.15 \end{array} \end{array}\right\} \qquad (163)$$

and estimates of $\hat{\gamma}_k$ or $(\hat{k})$ and $\hat{\delta}_\ell$ or $[\hat{\ell}]$ as shown in Table XXIXc. The residual is found, as usual, by taking from 213,708 the sum of products of these estimates times the appropriate right-hand constants of Equ. (159) to give 136.58. These results are summarized in Table XXIXc. There, there are shown tests of significance for both Treatments and Carries-over.

The Design and data may be simply fed into an appropriate program, as in the Appendix of this book and the complete analysis will be done. This last alternative is much the most in the spirit of these casual latter days, but it still seems wise to have some idea of what is going on.

Table XXIX - Youden rectangle with 2 missed lines

a. Data

	Week					
Group	1	2	3	4	5	Sum
I	(1) 82	(2) 68	(4) 70	(7) 78	(11) 81	379
II	(2) 74	(3) 65	(5) 65	(8) 65	(1) 75	344
III	(3) 74	(4) 57	(6) 54	(9) 60	(2) 69	314
IV	(4) 82	(5) 70	(7) 72	(10) 71	(3) 82	377
V	(5) 76	(6) 68	(8) 64	(11) 68	(4) 71	347
VI	(6) 72	(7) 76	(9) 60	(1) 74	(5) 68	350
VII	(8) 61	(9) 59	(11) 69	(3) 76	(7) 66	331
VIII	(9) 68	(10) 51	(1) 69	(4) 80	(8) 65	333
IX	(10) 60	(11) 66	(2) 55	(5) 60	(9) 66	307
Sum	649	580	578	632	643	3082

b. Analysis for direct Treatment alone

	$\widehat{(1)}$	$\widehat{(2)}$	$\widehat{(3)}$	$\widehat{(4)}$	$\widehat{(5)}$	$\widehat{(6)}$	$\widehat{(7)}$	$\widehat{(8)}$	$\widehat{(9)}$	$\widehat{(10)}$	$\widehat{(11)}$
Contr.	+4.15	+.41	+5.46	+2.27	-1.36	-1.15	+3.42	-5.18	-3.18	-9.29	+4.46
Adj. Mn.	72.46	68.72	73.77	70.58	66.95	67.16	71.73	63.13	65.13	59.02	72.77

Factors	d.f.	Residual Variability (Squares)
$\hat{\mu}$, Rows & Columns (control)	32	1111.20
Control factors plus Treatments	22	454.76

$$F_{10,22} = \frac{(1111.20 - 454.76)/10}{454.76/22} = 3.18^*$$

c. Analysis for direct Treatment and Carry-over

	$\widehat{(1)}$	$\widehat{(2)}$	$\widehat{(3)}$	$\widehat{(4)}$	$\widehat{(5)}$	$\widehat{(6)}$	$\widehat{(7)}$	$\widehat{(8)}$	$\widehat{(9)}$	$\widehat{(10)}$	$\widehat{(11)}$
Contr.	+3.19	+2.72	+11.66	+10.49	+3.68	-.19	+1.16	-11.08	-8.78	-12.37	-.46
Adj.Mn.	71.07	70.60	79.54	78.37	71.56	67.69	69.04	56.80	59.10	55.51	67.42

	$\widehat{[1]}$	$\widehat{[2]}$	$\widehat{[3]}$	$\widehat{[4]}$	$\widehat{[5]}$	$\widehat{[6]}$	$\widehat{[7]}$	$\widehat{[8]}$	$\widehat{[9]}$	$\widehat{[10]}$	$\widehat{[11]}$
Contr.	-3.53	-11.40	-5.97	+1.84	+5.28	+11.18	+8.09	+.58	+2.45	+.77	-9.29

Factors	d.f.	Residual Variability (Squares)
$\hat{\mu}$, Rows, Columns & Carries-over (control)	22	867.69
Control factors plus Treatments	12	136.58

$$F_{10,12} = \frac{(867.69 - 136.58)/10}{136.58/12} = 6.42^{**}$$

Factors	d.f.	Residual Variability (Squares)
$\hat{\mu}$, Rows, Columns & Treatments (control)	22	454.76
Control factors plus Carries-over	12	136.58

$$F_{10,12} = \frac{(454.76 - 136.58)/10}{136.58/12} = 2.80^{*}$$

In this situation of a Youden rectangle, $c < t$, the missing values disturb the adjusted mean, $\hat{\gamma}_k + \hat{\mu}$, from that gotten in Table XXVIII regardless of whether a given Treatment is missing or no. The situation differs from that obtaining for a latin square, as is discussed in connection with Table XXIX. The comparison of the results in Table XXIX with those from the full data of Table XXVIII show them to be much the same. The position we must necessarily take is that imperfect design must be accepted and can be handled very well, in a practical way. From this emerges a point, that we need not be too strict in design. Such possibilities are explored briefly in Chap. VIII.

Youden rectangles with missing cells - A situation with two arbitrarily chosen observations missing is shown in Table XXX, derived from Table XXVIII. Note that in Table XXX the entire Design, which was intended, is given, i.e., Treatment is given for the missing data cells. This assumes that the Treatment (8) or (10), as the case may be, was administered so that there will be a Carry-over if any subsequent Treatment was tried. Only the actual observation is missing. The situation is just the same as for the case of missing cells discussed in connection with latin squares. If such was not in fact the case, the resultant least squares equations would be a little modified. Allowance would have to be made for the fact that the cell following the miss would contain no Carry-over. Following previous lines, it is possible to set the least squares equations up as in Equ. (164). The equations are, of course, the same as Equ. (152) for the complete data except for $\hat{\mu}$ Rows VII and XI, Columns 2 and 5, Treatments (8) and (10) and Carries-over [6] and [7]. The least squares equations are as follows:

																																								=	Sum
53							-1				-1		-1			-1								-1		-1							-1	-1					=	3644
5	5																1	1		1			1				1	1	1		1			1					=	379
5		5															1	1	1		1			1					1	1		1			1				=	344
5			5															1	1	1		1			1					1	1		1			1			=	314
5				5															1	1	1		1			1					1	1		1			1		=	377
5					5															1	1	1		1			1					1	1		1			1	=	347
5						5											1				1	1	1		1			1					1	1		1			=	350
4							4						-1					1				1	1			1			1						1		1		=	284
5								5											1				1	1	1		1			1					1	1		1	=	331
5									5								1			1				1	1	1		1			1					1	1		=	333
5										5								1			1				1	1	1		1			1					1	1	=	307
	1	1	1	1	1	1	1	1	1	1	1																												=	0
11												11																											=	806
10							-1						10											-1										-1					=	636
11														11																									=	716
11															11																								=	771
												1	1	1	1	1																							=	0
5	1	1				1			1		1						5																		1	1	1	1	=	356
5	1	1	1				1			1								5										1								1	1	1	=	341
5		1	1	1				1			1								5									1	1								1	1	=	377
5	1		1	1	1				1											5								1	1	1								1	=	360
5		1		1	1	1				1											5							1	1	1	1								=	339
5			1		1	1	1				1											5							1	1	1	1							=	330
5	1			1		1	1	1															5							1	1	1	1						=	371
4		1			1			1	1				-1											4							1	1	1						=	255
5			1			1		1	1	1															5							1	1	1	1				=	313
4				1			1		1	1						-1										4								1	1	1			=	240
																	1	1	1	1	1	1	1	1	1	1	1												=	0
4	1					1			1		1	-1						1	1	1	1							4											=	296
4	1	1					1			1		-1							1	1	1	1							4										=	267
4		1	1					1			1	-1								1	1	1	1							4									=	252
4	1		1	1					1			-1									1	1	1	1							4								=	267
4		1		1	1					1		-1										1	1	1	1							4							=	271
3			1		1	1						-1				-1							1	1	1								3						=	200
3	1			1		1						-1	-1												1	1	1							3					=	212
4		1			1		1	1				-1					1								1	1	1								4				=	260
4			1			1		1	1			-1					1	1								1	1									4			=	263
4				1			1		1	1		-1					1	1	1								1										4		=	292
																												1	1	1	1	1	1	1	1	1	1	1	=	0

(The positions left empty contain zeroes, not shown)

First there must be considered the model, cut back to the lowest level, i.e., with only the control factors $\hat{\mu}$, Rows and Columns, and even they are no longer orthogonal. The elements of Treatment and Carry-over will be neglected, i.e., the columns (18th through 39th), involving $\hat{\gamma}_k$ and $\hat{\delta}_\ell$ of Equ. (164) and the corresponding rows must be dropped. The resultant estimates are:

$$\left.\begin{array}{c} \hat{\mu} = 68.76 \\ \begin{array}{ccccccccccc} \hat{\alpha}_1 & \hat{\alpha}_2 & \hat{\alpha}_3 & \hat{\alpha}_4 & \hat{\alpha}_5 & \hat{\alpha}_6 & \hat{\alpha}_7 & \hat{\alpha}_8 & \hat{\alpha}_9 & \hat{\alpha}_{10} & \hat{\alpha}_{11} \\ \hline +7.04 & +.04 & -5.96 & +6.64 & +.64 & +1.24 & +.97 & -2.56 & -2.16 & -7.36 & +1.46 \end{array} \\ \begin{array}{ccccc} \hat{\beta}_1 & \hat{\beta}_2 & \hat{\beta}_3 & \hat{\beta}_4 & \hat{\beta}_5 \\ \hline +4.51 & -5.06 & -3.67 & +1.33 & +2.89 \end{array} \end{array}\right\} \quad (165)$$

It now is necessary to find the residual squares on estimates of $\hat{\mu}$, Rows and Columns as control factors, which is:

$$\begin{aligned} & 253{,}838 - 68.76(3644) \\ & \quad - \{ + 7.04(379) + .04(344) - \ldots + 1.46(278)\} \\ & \quad - \{ + 4.51(806) - 5.06(636) - \ldots + 2.89(715)\} \\ & \qquad = 1568.38 \end{aligned} \qquad (166)$$

if the thing be carried out with accuracy, of the estimates to the fourth decimal place. It would have been impossible to use analysis of variance techniques. In order to find the further reduction due to Treatments it is necessary to consider Equ. (164) less the 29th through 39th columns and the corresponding rows. The resultant estimates are:

$$\left.\begin{array}{c} \hat{\mu} = 68.45 \\ \begin{array}{ccccccccccc} \hat{\alpha}_1 & \hat{\alpha}_2 & \hat{\alpha}_3 & \hat{\alpha}_4 & \hat{\alpha}_5 & \hat{\alpha}_6 & \hat{\alpha}_7 & \hat{\alpha}_8 & \hat{\alpha}_9 & \hat{\alpha}_{10} & \hat{\alpha}_{11} \\ \hline +4.77 & -.41 & -6.86 & +6.29 & +1.28 & +1.86 & +3.06 & -3.82 & +.93 & -5.44 & -1.67 \end{array} \\ \begin{array}{ccccc} \hat{\beta}_1 & \hat{\beta}_2 & \hat{\beta}_3 & \hat{\beta}_4 & \hat{\beta}_5 \\ \hline +4.83 & -5.09 & -3.36 & +1.64 & +1.97 \end{array} \end{array}\right\} \qquad (167)$$

and estimates of $\hat{\gamma}_k$ or $(\hat{k})$, as shown in Table XXXb. The residual is found, as usual, by taking from 253,838 the sum of products of these estimates times the appropriate right-hand constants of Equ. (164) to give 650.93. These results are summarized in Table XXXb.

Next there must be considered the model with only the control factors $\hat{\mu}$, Rows, Columns and Carries-over. The columns (18th through 28th) involving $\hat{\gamma}_k$ and corresponding rows of Equ. (164) must be dropped. The resultant estimates are:

$$\left.\begin{array}{c} \hat{\mu} = 68.72 \\ \begin{array}{ccccccccccc} \hat{\alpha}_1 & \hat{\alpha}_2 & \hat{\alpha}_3 & \hat{\alpha}_4 & \hat{\alpha}_5 & \hat{\alpha}_6 & \hat{\alpha}_7 & \hat{\alpha}_8 & \hat{\alpha}_9 & \hat{\alpha}_{10} & \hat{\alpha}_{11} \\ \hline +7.20 & +1.73 & -5.28 & +6.27 & +1.30 & +.34 & +.27 & -.95 & -4.33 & -8.15 & +1.60 \end{array} \\ \begin{array}{ccccc} \hat{\beta}_1 & \hat{\beta}_2 & \hat{\beta}_3 & \hat{\beta}_4 & \hat{\beta}_5 \\ \hline +4.55 & -5.39 & -3.63 & +1.37 & +3.11 \end{array} \\ \begin{array}{ccccccccccc} \hat{\delta}_1 & \hat{\delta}_2 & \hat{\delta}_3 & \hat{\delta}_4 & \hat{\delta}_5 & \hat{\delta}_6 & \hat{\delta}_7 & \hat{\delta}_8 & \hat{\delta}_9 & \hat{\delta}_{10} & \hat{\delta}_{11} \\ \hline +5.21 & -1.10 & -3.86 & -1.80 & -.12 & +1.71 & -2.94 & -3.17 & +.71 & +6.90 & -1.54 \end{array} \end{array}\right\} \qquad (168)$$

The residual is found, as usual, by taking from 253,838 the sum of products of these estimates times the appropriate right-hand constants of Equ. (164)

to give 1171.90. In order to find the further reduction due to Treatments it is necessary to consider the full model, with all factors, of Equ. (164). The resultant estimates are:

$$\left.\begin{array}{c}
\hat{\mu} = 68.77 \\
\begin{array}{ccccccccccc}
\hat{\alpha}_1 & \hat{\alpha}_2 & \hat{\alpha}_3 & \hat{\alpha}_4 & \hat{\alpha}_5 & \hat{\alpha}_6 & \hat{\alpha}_7 & \hat{\alpha}_8 & \hat{\alpha}_9 & \hat{\alpha}_{10} & \hat{\alpha}_{11} \\ \hline
+3.88 & +.53 & -11.15 & +1.29 & -1.02 & -2.57 & +5.98 & +1.37 & +1.19 & -1.09 & +1.60
\end{array} \\
\begin{array}{ccccc}
\hat{\beta}_1 & \hat{\beta}_2 & \hat{\beta}_3 & \hat{\beta}_4 & \hat{\beta}_5 \\ \hline
+4.50 & -4.96 & -3.68 & +1.32 & +2.81
\end{array}
\end{array}\right\} \qquad (169)$$

and estimates of $\hat{\gamma}_k$ or $(\hat{k})$ and $\hat{\delta}_\ell$ or $[\hat{\ell}]$, as shown in Table XXXc. The residual is found, as usual, by taking from 253,838 the sum of products of these estimates times the appropriate right-hand constants of Equ. (164) to give 207.66. These results are summarized in Table XXXc. There, there are shown tests of significance for both Treatments and Carries-over.

Unfortunately, one cannot, as for previous problems, in practice avoid the effort required to set up the foregoing equations by having an electronic computer do so, after it has been given the Design and the data, in the electronic program of the Appendix. The problem is discussed there. Briefly, the program would have to be extended.

Table XXX - Youden rectangle with 2 missed observations

a. Data

Group	Week 1	2	3	4	5	Sum
I	(1) 82	(2) 68	(4) 70	(7) 78	(11) 81	379
II	(2) 74	(3) 65	(5) 65	(8) 65	(1) 75	344
III	(3) 74	(4) 57	(6) 54	(9) 60	(2) 69	314
IV	(4) 82	(5) 70	(7) 72	(10) 71	(3) 82	377
V	(5) 76	(6) 68	(8) 64	(11) 68	(4) 71	347
VI	(6) 72	(7) 76	(9) 60	(1) 74	(5) 68	350
VII	(7) 79	(8) NR	(10) 58	(2) 75	(6) 72	284
VIII	(8) 61	(9) 59	(11) 69	(3) 76	(7) 66	331
IX	(9) 68	(10) 51	(1) 69	(4) 80	(8) 65	333
X	(10) 60	(11) 66	(2) 55	(5) 60	(9) 66	307
XI	(11) 78	(1) 56	(3) 80	(6) 64	(10) NR	278
Sum	806	636	716	771	715	3644

b. Analysis for direct Treatment alone

	$(\hat{1})$	$(\hat{2})$	$(\hat{3})$	$(\hat{4})$	$(\hat{5})$	$(\hat{6})$	$(\hat{7})$	$(\hat{8})$	$(\hat{9})$	$(\widehat{10})$	$(\widehat{11})$
Contr.	+1.66	+.73	+8.25	+2.27	-1.36	-1.98	+3.32	-5.46	-3.18	-9.16	+4.93
Adj.Mn.	70.11	69.18	76.70	70.72	67.09	66.47	71.77	62.99	65.27	59.29	73.38

Factors	d.f.	Residual Variability (Squares)
$\hat{\mu}$, Rows & Columns (control)	38	1568.38
Control factors plus Treatments	28	650.93

$$F_{10,28} = \frac{(1568.38 - 650.93)/10}{650.93/28} = 3.95^{**}$$

c. Analysis for direct Treatment and Carry-over

	$\widehat{(1)}$	$\widehat{(2)}$	$\widehat{(3)}$	$\widehat{(4)}$	$\widehat{(5)}$	$\widehat{(6)}$	$\widehat{(7)}$	$\widehat{(8)}$	$\widehat{(9)}$	$\widehat{(10)}$	$\widehat{(11)}$
Contr.	+3.31	+1.83	+12.31	+9.82	+2.56	+.40	+.97	-11.40	-8.43	-12.33	+.96
Adj.Mn.	72.08	70.60	81.08	78.59	71.33	69.17	69.74	57.37	60.34	56.44	69.73

	$\widehat{[1]}$	$\widehat{[2]}$	$\widehat{[3]}$	$\widehat{[4]}$	$\widehat{[5]}$	$\widehat{[6]}$	$\widehat{[7]}$	$\widehat{[8]}$	$\widehat{[9]}$	$\widehat{[10]}$	$\widehat{[11]}$
Contr.	-1.30	-9.49	-6.17	+2.17	+4.80	+11.54	+7.48	-.23	+2.45	-1.09	-10.17

Factors	d.f.	Residual Variability (Squares)
$\hat{\mu}$, Row, Columns & Carries-over (control)	28	1171.90
Control factors plus Treatments	18	207.66

$$F_{10,18} = \frac{(1171.90 - 207.66)/10}{207.66/18} = 8.36^{***}$$

Factors	d.f.	Residual Variability (Squares)
$\hat{\mu}$, Rows, Columns & Treatments (control)	28	650.93
Control factors plus Carries-over	18	207.66

$$F_{10,18} = \frac{(650.93 - 207.66)/10}{207.66/18} = 3.84^{**}$$

In this situation of a Youden rectangle, $c < t$, the missing values disturb the adjusted mean, $\hat{\gamma}_k + \hat{\mu}$, from that gotten in Table XXVIII regardless of whether a given Treatment is missing or no. The situation differs from that obtaining for a latin square, as is discussed in connection with Table XXIX. The comparison of the results in Table XXX with those from the full data of Table XXVIII shows them to be very much the same, as might be expected because the data are very much so.

VII. Analysis of data from various special kinds of Youden

Page

Special Youden Designs 236

Double Youden, t x c x 2t 238

Paired Youdens, 2 (t x c x t) 247

Repeated Youdens 252

Missing data situations 259

Designs more or less Youden 266

Breaking Treatment variability up 275

Design with several sections 281

Special Youden Designs - The discussion of analysis, so far, has been a detailed one for the cases of the latin square and the single basic Youden rectangle with $c < t$ and $r = t$. The only serious practical complication has been that a Design may be incomplete by some Rows. In order to deal with the Youdens generally it is necessary to consider, in some detail, extensions, beyond this realm. First, there are Designs, already indicated, where the number of Rows, some multiple of t, the number of Treatments but the fill is an integer and $c \leq t$. Secondly, there are two outlying types of design.

The Designs with number of Rows, some multiple of t, are of 3 types:

a. Multiple Youdens where the number of Rows necessary to get integer fill is

$$r = gt \; . \tag{170}$$

For this type, a number of Designs were shown in Table XVIII, i.e., cases where $g = 2$. There, of course, exist more cases where $g > 2$ is some integer. Here we consider only double Youdens, except for paired comparisons in Chap. VIII on that subject. This type of Design arises only for $c < t$; there is, of course, no analogy in the case of the latin squares where the fill is always t.

b. Balanced Youdens where the number of Youdens necessary to get all $t - 1$ Treatments preceding (or for that matter following) a given Treatment requires gt Rows although each successive t Rows is a true Youden (with integer fill) and with unrepeated Change-over, in its own right. Some Designs of this type are shown in Table XVII. Here we consider only paired,

$g = 2$, cases. Of course, there can exist cases where $g > 2$ is some integer. This type of Design arises only for $c < t$. When $c = t$, in the latin square, if a Design can be written with unrepeated Change-over, it is necessarily balanced.

c. Various repeated Designs are very commonly used. Thus each successive t Rows may be a repetition of the same single Youden. It may also be a latin square repeated. In practice g is often some number of magnitude 5. It seems most practical to make a simple repetition although the literature contains examples where a succession of Designs is used, that is, each t Row is a fresh Youden in its own right. Such richness may delight the professional statistician but must dismay the practical experimentalist. It may be practical to repeat the double Youdens, the paired Youdens, as above.

When, however, it comes to more complicated matters like finding the significance of treatment effects (even without allowance for Carry-over), it will be assumed that the reader have an electronic computer available and that he will use least squares equations, as in previous discussion, in some way. He may simply feed in the Design and the results if circumstances are favorable. To set forth explicit statements such as were put out for the single unrepeated Youden is intolerable. Of course, when Carry-over is indeed similar equipment and policy are assumed.

An odd Design which may be termed the near-Youden will be presented and discussed. A common practical situation where an experiment is done in several blocks will be indicated but not fully treated.

Double Youden, $t \times c \times 2t$ - Consider first the double Youden, $t \times c \times 2t$. Such an example is presented in Table XXXI, which is concerned with satisfaction reported by 18 Men on each of some 4 Treatments that each was put under successively. It is a 9x4x18. For such multiple, balanced or repeated Youdens, the total fill or number of times that the comparison table is filled, in the sense of Equ. (2), is

$$f = gc(c - 1)/(t - 1) . \tag{171}$$

Consider next the estimation of direct treatment effects. The underlying assumptions are the same as those for a single Youden, $t \times c \times t$, with which we started the consideration of analysis of data. The estimate of the effect of the k^{th} Treatment is

$$\hat{\gamma}_k = (c\Sigma y_{ijk} - \Sigma T_i)/ft \tag{172}$$

where y_{ijk} is any observations under Treatment k and where T_i is the sum of observations for any Row in which k occurs. This is all very much as in Equ. (112) for one single Youden except that the observations y_{ijk} under k occur gc rather than c times. The same statements may be made for balanced and repeated Youdens. Generally, it is impossible to reverse the roles of Rows and Treatments, as was done for single Youdens, so that there are no easily calculated estimates of Row effects.

For double Youdens it is impractical to put in a 0^{th} conditioning Period of the type that is sometimes used with latin squares. It is even less practical than for a single Youden where it does little good. For double Youdens such a step would mean that a given Treatment had its own Carry-over twice associated with it whereas such other Carries-over as are associated are so but once.

The estimate for Treatment(1), directly, without allowance for Carry-over, following Equ. (171) and (172) is

$$\hat{\gamma}_1 \text{ or } (\hat{1}) = \frac{4(y_{111} + y_{221} + \dots + y_{731} + y_{841}) - (T_1 + T_2 + \dots + T_8)}{27}$$

$$= [4\{2.5+2.1+2.1+1.6+1.7+2.3+2.1+2.1\}$$

$$- \{12.5+10.1+9.5+7.8+7.3+10.9+10.6+10.2\}]/27$$

$$= -.48 \qquad (173)$$

In order to get the effect of Treatment or Material (1), from Equ. (129), we take 4 times the results at (1) at each of the 8 occasions when it occurs less the sum for each of the 8 Rows in which it occurs all divided by 27 to get estimates as are shown in Table XXXIb. This quantity may be called the treatment contribution, i.e., the amount that the Treatment differs from the average of all Treatments. To this each such quantity there may be added $\hat{\mu} = 2.57$ to give what may be referred to as the adjusted mean which is often a convenient practical form in which to report effect of Treatments.

The business of analysis again involves setting up least squares equations for the effects, μ, α_i, β_j, γ_k and δ_ℓ appropriate for various levels of the models of Equ. (7) and Equ. (43) and finding the variability variously residual. The handiest thing to do seems to be to set up the full equations, as in Equ. (48), previously, and then cut back to the lesser situations by judiciously dropping rows and columns. Examining Table XXXIa, it is at once apparent that the grand total 1958.88 contains all effects of Rows, Columns, Treatments and Carries-over equally, so that we may write the first line of the grand set of Equ. (175). When it comes to estimating the effect of Rows, it is necessary to recognize that their totals contain no effect of Column, in the sense that they contain them all equally and remembering Equ. (9). On the other hand the total for Row contains much effect of Treatment. Thus for Row, or Man I, there are present Treatments (1), (2), (4) and (8). It would be mischievous to convert this statement into one that it lack Treatments (3), (5), (6) and (7). The total for Row I must also contain 3 Carries-over. Thus there may be written the second line of Equ. (175). Others may be written likewise, as shown in the following lines. In the 19th line the corresponding statement is not made but rather there is, again, an appeal to the condition of Equ. (9), to avoid the algebraic problem of singularity, if Equ. (145) are treated as a set of simultaneous equations. For Columns and equations are:

$$\left.\begin{aligned} 18\hat{\beta}_1 + 18\hat{\mu} &= 46.7 \\ 18\hat{\beta}_2 + 18\hat{\mu} &= 45.3 \\ 18\hat{\beta}_3 + 18\hat{\mu} &= 46.0 \\ \hat{\beta}_1 + \hat{\beta}_2 + \hat{\beta}_3 + \hat{\beta}_4 &= 0 \end{aligned}\right\} \qquad (174)$$

These are not included in Equ. (175) because it is somewhat bulky anyhow.

Treatments and Carries-over are extensively and intimately confounded with each other and with Rows, as set forth in Equ. (175). In addition there is the peculiarity that each Carry-over is confounded with Column 1, although it is free of them! We have:

$\hat{\mu}$	$\hat{\alpha}_1$	$\hat{\alpha}_2$	$\hat{\alpha}_3$	$\hat{\alpha}_4$	$\hat{\alpha}_5$	$\hat{\alpha}_6$	$\hat{\alpha}_7$	$\hat{\alpha}_8$	$\hat{\alpha}_9$	$\hat{\alpha}_{10}$	$\hat{\alpha}_{11}$	$\hat{\alpha}_{12}$	$\hat{\alpha}_{13}$	$\hat{\alpha}_{14}$	$\hat{\alpha}_{15}$	$\hat{\alpha}_{16}$	$\hat{\alpha}_{17}$	$\hat{\alpha}_{18}$	$\hat{\beta}_1$	$\hat{\gamma}_1$	$\hat{\gamma}_2$	$\hat{\gamma}_3$	$\hat{\gamma}_4$	$\hat{\gamma}_5$	$\hat{\gamma}_6$	$\hat{\gamma}_7$	$\hat{\gamma}_8$	$\hat{\gamma}_9$	$\hat{\delta}_1$	$\hat{\delta}_2$	$\hat{\delta}_3$	$\hat{\delta}_4$	$\hat{\delta}_5$	$\hat{\delta}_6$	$\hat{\delta}_7$	$\hat{\delta}_8$	$\hat{\delta}_9$	=	Sum
72																																						=	184.8
4	4																			1	1		1				1		1	1		1						=	12.5
4		4																			1	1		1				1		1	1		1					=	10.7
4			4																	1		1	1		1						1	1		1				=	10.1
4				4																	1		1	1		1						1	1		1			=	8.1
4					4																	1		1	1		1						1	1		1		=	9.3
4						4																	1		1	1		1						1	1		1	=	11.8
4							4													1				1		1	1		1						1	1		=	9.5
4								4													1				1		1	1		1						1	1	=	13.8
4									4											1		1				1		1	1		1						1	=	7.8
4										4										1	1	1			1				1		1			1				=	7.3
4											4										1	1	1			1				1		1			1			=	7.0
4												4										1	1	1			1				1		1			1		=	10.8
4													4										1	1	1			1				1		1			1	=	11.8
4														4						1				1	1	1			1				1		1			=	10.9
4															4						1				1	1	1			1				1		1		=	12.8
4																4						1				1	1	1			1				1		1	=	9.8
4																	4			1			1				1	1	1			1				1		=	10.6
	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1																				=	.0
18																			18																			=	46.7
8	1		1				1		1	1				1			1	1		8										1		1	1	1		1	1	=	16.5
8	1	1		1				1		1	1				1			1			8								1		1		1	1	1		1	=	21.8
8		1	1		1				1	1	1	1				1						8							1	1		1		1	1	1		=	15.3
8	1		1	1		1					1	1	1				1						8							1	1		1		1	1	1	=	21.8
8		1		1	1		1					1	1	1				1						8					1		1	1		1		1	1	=	19.7
8			1		1	1		1		1			1	1	1										8				1	1		1	1		1		1	=	24.4
8				1		1	1		1		1			1	1	1										8			1	1	1		1	1		1		=	17.2
8	1				1		1	1				1			1	1	1										8			1	1	1		1	1		1	=	23.8
																				1	1	1	1	1	1	1	1	1										=	.0
6	1						1		1	1				1			1		-2		1	1		1	1	1		1	6									=	15.3
6	1	1						1			1				1			1	-2	1		1	1		1	1	1			6								=	16.8
6		1	1						1	1		1				1			-2		1		1	1		1	1	1			6							=	15.4
6	1		1	1							1		1				1		-2	1		1		1	1		1	1				6						=	14.1
6		1		1	1							1		1				1	-2	1	1		1		1	1		1					6					=	15.5
6			1		1	1				1			1		1				-2	1	1	1		1		1	1							6				=	13.9
6				1		1	1				1			1		1			-2		1	1	1		1		1	1							6			=	15.4
6					1		1	1				1			1		1		-2	1		1	1	1		1		1								6		=	15.2
																													1	1	1	1	1	1	1	1	1	=	.0

(175).

(The positions left empty contain zeroes, not shown)

The analysis is, of course, made in a manner similar to that for a single Youden. For the model without Carry-over, the variability residual on the orthogonal control factors, Rows and Columns can be gotten by the procedures of analysis of variance. Such results are shown in Table XXXIb. When Treatment is included the situation becomes more involved since it is necessarily confounded with Rows. It is necessary to solve simultaneous equations which can be gotten from Equ. (175) by dropping the lines and columns involving Change-over, i.e., the 30th through 38th of each. Working with these equations one gets the analysis of Table XXXIb. The appropriate design-data program for electronic computer may be used. In the actual equations being used it is very important to remember that in getting residual squares one uses in connection with the estimate for $\hat{\alpha}_{18}$ not the conditioning equation above but a suppressed equation formed just as for the other Rows with a right-hand constant of 10.2. Similarly, in conjunction with the estimate for $\hat{\gamma}_9$ there is used the right-hand constant of 24.3 from a suppressed equation. Further, by dropping the 21st through the 29th line there may be found estimates of $\hat{\mu}$, $\hat{\alpha}_i$, $\hat{\beta}_j$ and $\hat{\delta}_\ell$ together with the variability residual thereon, i.e., the 6.9100 of Table XXXIc. Then by solving the entire body of Equ. (175) the estimates are as in that table and the residual variability is 2.2830. There has been added the test of significance for Carries-over, *per se*.

Table XXXI - Satisfaction by Men, Periods and Treatments - The double Youden 9x4x18

a. Data

Period

Man	1	2	3	4	Sum
I	(1) 2.5	(2) 3.5	(4) 3.4	(8) 3.1	12.5
II	(2) 2.3	(3) 2.5	(5) 2.5	(9) 3.4	10.7
III	(3) 2.2	(4) 3.1	(6) 2.7	(1) 2.1	10.1
IV	(4) 2.2	(5) 1.8	(7) 1.6	(2) 2.5	8.1
V	(5) 2.3	(6) 2.6	(8) 2.5	(3) 1.9	9.3
VI	(6) 3.2	(7) 2.0	(9) 3.3	(4) 3.3	11.8
VII	(7) 2.5	(8) 2.7	(1) 2.1	(5) 2.2	9.5
VIII	(8) 3.7	(9) 3.0	(2) 3.3	(6) 3.8	13.8
IX	(9) 2.6	(1) 1.6	(3) 1.9	(7) 1.7	7.8
X	(1) 1.7	(6) 2.2	(3) 1.3	(2) 2.1	7.3
XI	(2) 2.3	(7) 1.6	(4) 1.8	(3) 1.3	7.0
XII	(3) 2.2	(8) 3.1	(5) 2.6	(4) 2.9	10.8
XIII	(4) 2.4	(9) 3.1	(6) 3.4	(5) 2.9	11.8
XIV	(5) 3.0	(1) 2.3	(7) 2.5	(6) 3.1	10.9
XV	(6) 3.4	(2) 3.1	(8) 3.4	(7) 2.9	12.8
XVI	(7) 2.4	(3) 2.0	(9) 2.9	(8) 2.5	9.8
XVII	(8) 2.8	(4) 2.7	(1) 2.1	(9) 3.0	10.6
XVIII	(9) 3.0	(5) 2.4	(2) 2.7	(1) 2.1	10.2
Sum	46.7	45.3	46.0	46.8	184.8
Mean	2.59	2.52	2.56	2.60	2.57
Sum Sq.	125.39	119.53	124.88	129.10	1958.88

b. Analysis for direct Treatment alone

	$\widehat{(1)}$	$\widehat{(2)}$	$\widehat{(3)}$	$\widehat{(4)}$	$\widehat{(5)}$	$\widehat{(6)}$	$\widehat{(7)}$	$\widehat{(8)}$	$\widehat{(9)}$
Contr.	-.48	+.18	-.43	+.17	-.09	+.36	-.33	+.23	+.40
Adj. Mn.	2.09	2.75	2.14	2.74	2.48	2.93	2.24	2.80	2.97

Factors	d.f.	Residual Variability (Squares)
$\hat{\mu}$, Rows & Columns (control)	51	9.0988
Control factors plus Treatment	43	2.8211

$$F_{8,43} = \frac{(9.0988 - 2.8211)/8}{2.8211/43} = 11.96^{***}$$

c. Analysis for direct Treatment with allowance for Carry-over

	$(\hat{1})$	$(\hat{2})$	$(\hat{3})$	$(\hat{4})$	$(\hat{5})$	$(\hat{6})$	$(\hat{7})$	$(\hat{8})$	$(\hat{9})$
Contr.	-.47	+.20	-.39	+.08	-.05	+.31	-.34	+.22	+.44
Adj. Mn.	2.10	2.77	2.18	2.65	2.52	2.88	2.23	2.79	3.01

	$[\hat{1}]$	$[\hat{2}]$	$[\hat{3}]$	$[\hat{4}]$	$[\hat{5}]$	$[\hat{6}]$	$[\hat{7}]$	$[\hat{8}]$	$[\hat{9}]$
Contr.	-.01	+.22	+.08	-.19	+.06	-.16	-.01	-.09	+.09

Factors	d.f.	Residual Variability (Squares)
$\hat{\mu}$, Rows, Columns & Carries-over (control)	43	6.9100
Control factors plus Treatment	35	2.2830

$$F_{8,35} = \frac{(6.9100 - 2.2830)/8}{2.2830/35} = 8.87^{***}$$

Factors	d.f.	Residual Variability (Squares)
$\hat{\mu}$, Rows, Columns & Treatments (control)	43	2.8211
Control factors plus Carries-over	35	2.2830

$$F_{8,35} = \frac{(2.8211 - 2.2830)/8}{2.2830/43} = 1.27 \text{ N.S.}$$

d. Results by Treatment and by previous Treatment--a crude analysis

After	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	Sum	Adj. Mean	Contr.
	Treatment											
(1)		3.5	1.9		2.2	2.2	2.5		3.0	15.3	2.55	-.01
(2)	2.1		2.5	3.4		3.8	1.6	3.4		16.8	2.90	+.34
(3)		2.1		3.1	2.5		1.7	3.1	2.9	15.4	2.44	-.12
(4)	2.1		1.3		1.8	2.7		3.1	3.1	14.1	2.32	-.24
(5)	2.3	2.7		2.9		2.6	1.6		3.4	15.5	2.53	-.03
(6)	2.1	3.1	1.3		2.9		2.0	2.5		13.9	2.50	-.06
(7)		2.5	2.0	1.8		3.1		2.7	3.3	15.4	2.38	-.18
(8)	2.1		1.9	2.7	2.6		2.9		3.0	15.2	2.72	+.16
(9)	1.6	3.3		3.3	2.4	3.4		2.5		16.5	2.65	+.09
Bk. grd.	2.5	2.3	2.2	2.2	2.3	3.2	2.5	3.7	2.6			
Mn.	1.7	2.3	2.2	2.4	3.0	3.4	2.4	2.8	3.0	46.7		
Sum	16.5	21.8	15.3	21.8	19.7	24.4	17.2	23.8	24.3	184.8		
Adj. Mn.	2.02	2.76	1.90	2.69	2.48	3.05	2.11	2.99	3.08			
Contr	-.55	+.19	-.67	+.12	-.09	+.48	-.46	+.42	+.51			

In Table XXXId there are shown the data arranged according to Treatment and preceding Treatment. This is the kind of crude analysis for direct Treatment effects and Carry-over previously suggested. The contributions, $(\hat{k})$ and $[\hat{\ell}]$ have been estimated, without allowance for Row effects, which have been simply neglected, to the great simplification of matters. The contributions were found by iterative procedure, as in Chap. IX. In a case like the present, where Treatments and Carries-over are but little confounded, the matter goes quite rapidly. It may, further, be observed that with the table so nearly filled and so many Rows to iron out, the estimates of effect of Treatment are fairly good, i.e., as compared with those of Table XXXIc.

The correlation between estimates of Treatment, proper, and Carry-over in Table XXXIc is -.08. This is so in spite of the considerable effect of Treatment. Indeed, the effect of Carries-over is very slight as shown by its specific test of significance that terminates XXXIc. As a result, the effect of Treatments is determined there with less reliability than without Carry-over in Table XXXIb; the strength of the experiment has been dissipated in the vain calculation of Carry-over. Presumably in such an experiment one should fall back on the more simple analysis without Carry-over.

Paired Youdens 2(t x c x t) - Let us now turn to the analysis for paired Youdens, 2(t x c x t) , as discussed in Table XVII. As an example, there are the results shown in Table XXXII. This was a 2-week test on Treatments, (1), (2), and (3). Six large groups of men received them with results as shown in Table XXXIIa. The analysis of the present data, as shown in Table XXXIIb and c, will be confined to the formation of treatment and carry-over estimates. Significance can hardly be calculated since the degrees of freedom (d.f.) are so few for the residual squares. This does not, however, inhibit the estimation of the effects. They are really the most important thing in an experiment and the statistical analysis. There is a tendency to stress significance too much--probably because it is a good deal harder to find. For the estimation of treatment effects, without allowance for Carry-over, we may follow Equ. (172) to get results such as

$$\begin{aligned}\hat{\gamma}_1 \text{ or } (\hat{1}) &= \frac{2(y_{111} + y_{321} + y_{411} + y_{521}) - (T_1 + T_2 + T_3 + T_4)}{6} \\ &= [2\{41.0 + 43.3 + 51.0 + 48.0\} - \{92.3 + 90.0 + 96.3 + 94.7\}]/6 \\ &= -1.12 \quad . \end{aligned} \qquad (176)$$

For the more extensive analysis, when Carry-over is involved, it is necessary to set up least squares equations, as in Equ. (177) or to use an electronic program, as shown in the Appendix.

Since the present Design happens to be not only a paired Youden but a paired comparison or single Change-over case, it belongs, in a sense, to Chap. VIII, devoted to such situations. There it will be pointed out that special techniques may be used in its analysis. Here, however, we shall proceed, using the general methods built up in the preceding discussion and directed at cases with, generally, more than a single Change-over.

In order to estimate the Treatment and Carry-over simultaneously it remains necessary to set up the complete set of simultaneous equations as previously. Examining Table XXXIa, it is at once apparent that the grand total contains all effects of Rows, Columns, Treatments and Carries-over equally, so that we may write the first line of the grand set of Equ. (177). When it comes to estimating the effect of Rows, it is necessary to recognize that their totals contain no effect of Column, in the sense that they contain them all equally and remembering Equ. (9). On the other hand the total for Row contains much effect of Treatment and Carry-over. Following the lines used previously, for larger cases, there arise the equations,

$$\left.\begin{array}{ccccccccccccccccc}
\hat{\mu} & \hat{\alpha}_1 & \hat{\alpha}_2 & \hat{\alpha}_3 & \hat{\alpha}_4 & \hat{\alpha}_5 & \hat{\alpha}_6 & \hat{\beta}_1 & \hat{\beta}_2 & \hat{\gamma}_1 & \hat{\gamma}_2 & \hat{\gamma}_3 & \hat{\delta}_1 & \hat{\delta}_2 & \hat{\delta}_3 & = & \text{Sum} \\
\hline
12 & & & & & & & & & & & & & & & = & 554.9 \\
2 & 2 & & & & & & & & & & -1 & 1 & & & = & 92.3 \\
2 & & 2 & & & & & & & -1 & & & & 1 & & = & 94.3 \\
2 & & & 2 & & & & & & & -1 & & & & 1 & = & 90.0 \\
2 & & & & 2 & & & & & & -1 & & 1 & & & = & 96.3 \\
2 & & & & & 2 & & & & & & -1 & & 1 & & = & 94.7 \\
 & 1 & 1 & 1 & 1 & 1 & 1 & & & & & & & & & = & .0 \\
6 & & & & & & & 6 & & & & & & & & = & 281.4 \\
 & & & & & & & 1 & 1 & & & & & & & = & .0 \\
4 & & -1 & & & -1 & & & & 4 & & & -1 & & & = & 183.3 \\
4 & & & -1 & -1 & & & & & & 4 & & & -1 & & = & 191.3 \\
 & & & & & & & & & 1 & 1 & 1 & & & & = & .0 \\
2 & 1 & & & 1 & & & & 2 & -1 & & & 2 & & & = & 96.6 \\
2 & & 1 & & & 1 & & & 2 & & -1 & & & 2 & & = & 91.3 \\
 & & & & & & & & & & & & 1 & 1 & 1 & = & .0
\end{array}\right\} \qquad (177)$$

(The positions left empty contain zeroes, not shown.)

The only analysis necessary on the basis of Equ. (177) is its solution to provide the estimates of Treatment and Carry-over, as shown in Table XXXIIc.

Table XXXII - Satisfaction under three kinds of Treatment in two Periods

2(3x2x3)

a. Data

Group	Period 1	 2	Sum
I	(1) 41.0	(2) 51.3	92.3
II	(2) 51.0	(3) 43.3	94.3
III	(3) 46.7	(1) 43.3	90.0
IV	(1) 51.0	(3) 45.3	96.3
V	(2) 46.7	(1) 48.0	94.7
VI	(3) 45.0	(2) 42.3	87.3
Sum	281.4	273.5	554.9

b. Analysis for direct Treatment alone

	$(\hat{1})$	$(\hat{2})$	$(\hat{3})$
Contr.	-1.12	+2.33	-1.22
Adj. Mn.	45.12	48.57	45.02

Analysis for significance hardly possible with so few observations

c. Analysis for direct Treatment with allowance for Carry-over

	$(\hat{1})$	$(\hat{2})$	$(\hat{3})$
Contr.	+2.77	+5.57	-8.33
Adj. Mn.	49.01	51.81	37.91

	$[\hat{1}]$	$[\hat{2}]$	$[\hat{3}]$
Contr.	+7.77	+6.47	-14.23

Analysis for significance hardly possible.

d. Results by Treatment and by previous Treatment--a crude analysis

	Treatment				Adj.	
After	(1)	(2)	(3)	Sum	Mean	Contr.
(1)		51.3	45.3	96.6	48.45	+2.87
(2)	48.0		43.3	91.3	46.56	+ .98
(3)	43.3	42.3		85.6	41.74	-3.84
Bk.grd.	41.0	51.0	46.7			
	51.0	46.7	45.0	281.4		
Sum	183.3	191.3	180.3	554.9		
Adj. Mn.	46.54	48.07	44.12			
Contr.	+.30	+1.83	-2.12			

In Table XXXIId, there is shown the simple calculation, as on a desk calculator, for treatment and carry-over effects, when Rows are ignored. It goes fairly fast by iteration. The results agree badly with the more exact results of Table XXXIIc.

The circumstance that it is hardly practical to make tests of significance in Table XXXII is not basic but arises from the paucity of data. Such a Design could be fully analyzed if it were sufficiently repeated. Thus, if it were repeated once, i.e., involved 12 Groups, there would be 7 degrees of freedom for the residual variability. With 18 Groups there would be 13 such degrees of freedom.

Repeated Youdens - There must be some question as to how we should handle the very common experimental situation when some Youden rectangle, $c < t$, is laid down repeatedly. Thus we may consider the data, as in Table XXXIIIa, a 4x3x4 (Yates rectangle) filled thrice-over on 12 Groups of subjects. Each Group was subject to 3 successive Treatments. Since the Design was repeated 3 times $g = 3$. Then from Equ. (134),

$$(\hat{k}) = (3\Sigma y_{ijk} - \Sigma T_i)/24 \tag{178}$$

and the estimates for Treatments, with only them and not Carries-over, are:

Treatment	$3\Sigma y_{ijk}$	ΣT_i	$(\hat{k})$
(1)	1830	1788	+1.75
(2)	1752	1730	-1.17
(3)	1719	1709	+.42
(4)	1800	1824	-1.00
Sum	7101	7101	.00

(179)

As previously, in presenting such results, for practical consideration, it may be wise to produce not such estimates of treatment effects but them added to the overall effect, $\hat{\mu} = 65.8$. Thus the "adjusted mean" for Treatment (1) is 65.8 + 1.8 = 67.6 in the sense that this is the mean anticipated if Treatment (1) had appeared in all Rows. Accordingly, the, say, adjusted averages for the Treatments are as shown in Table XXXIIIb.

As will transpire, the present problem is a peculiar one in that it would not yield the analysis gotten from the other Designs in a general way. This was not discovered until the experiment was run. The problem will be discussed in some detail below. For the moment it may be supposed, and in actual handling it was supposed, that the business of analysis again involves setting up least squares equations for the effects, μ , α_i , β_j , γ_k and δ_ℓ appropriate for various levels of the models of Equ. (7) and Equ. (43) and finding the variability variously residual. The handiest thing to do seems to be to set up the full equations, as in Equ. (48), previously, and then cut back to the lesser situations by judiciously dropping rows and columns. Examining Table XXXIIIa, it is at once apparent that the grand total 2367 contains all effects of Rows, Columns, Treatments and Carries-over equally, so that we may write the first line of the grand set of Equ. (181). Hence

$$\hat{\mu} = 2367/36 = 65.75 \quad . \tag{180}$$

When it comes to estimating the effect of Rows, it is necessary to recognize that their totals contain much effect of Treatment and Carry-over. Thus there may be written the second line of Equ. (181). Others may be written likewise, as shown in the following lines. The equations are much like those of the earlier, complete (no missing Rows) cases except that an account of the repetition of pattern the confounding in the first 12 lines is repeated. The total matrix is as follows:

$$\left.\begin{array}{cccccccccccccccccccccccc}
\hat{\mu} & \hat{\alpha}_1 & \hat{\alpha}_2 & \hat{\alpha}_3 & \hat{\alpha}_4 & \hat{\alpha}_5 & \hat{\alpha}_6 & \hat{\alpha}_7 & \hat{\alpha}_8 & \hat{\alpha}_9 & \hat{\alpha}_{10} & \hat{\alpha}_{11} & \hat{\alpha}_{12} & \hat{\beta}_1 & \hat{\gamma}_1 & \hat{\gamma}_2 & \hat{\gamma}_3 & \hat{\gamma}_4 & \hat{\delta}_1 & \hat{\delta}_2 & \hat{\delta}_2 & \hat{\delta}_4 & = & \text{Sum} \\
\hline
36 &&&&&&&&&&&&&&&&&&&&&& = & 2367 \\
3 & 3 &&&&&&&&&&&&&&& -1 && 1 & 1 &&& = & 231 \\
3 && 3 &&&&&&&&&&&&&&& -1 && 1 & 1 && = & 236 \\
3 &&& 3 &&&&&&&&&&& -1 &&&&&& 1 & 1 & = & 206 \\
3 &&&& 3 &&&&&&&&&&& -1 &&& 1 &&& 1 & = & 172 \\
3 &&&&& 3 &&&&&&&&&&& -1 && 1 & 1 &&& = & 225 \\
3 &&&&&& 3 &&&&&&&&&&& -1 && 1 & 1 && = & 142 \\
3 &&&&&&& 3 &&&&&&& -1 &&&&&& 1 & 1 & = & 208 \\
3 &&&&&&&& 3 &&&&&&& -1 &&& 1 &&& 1 & = & 219 \\
3 &&&&&&&&& 3 &&&&&&& -1 && 1 & 1 &&& = & 202 \\
3 &&&&&&&&&& 3 &&&&&&& -1 && 1 & 1 && = & 165 \\
3 &&&&&&&&&&& 3 &&& -1 &&&&&& 1 & 1 & = & 165 \\
& 1 & 1 & 1 & 1 & 1 & 1 & 1 & 1 & 1 & 1 & 1 & 1 &&&&&&&&&& = & 0 \\
12 &&&&&&&&&&&&& 12 &&&&&&&&& = & 769 \\
9 &&& -1 &&&& -1 &&&& -1 &&& 9 &&&&&& 3 & 3 & = & 610 \\
9 &&&& -1 &&&& -1 &&&& -1 &&& 9 &&& 3 &&& 3 & = & 584 \\
9 & -1 &&&& -1 &&&& -1 &&&&&&& 9 && 3 & 3 &&& = & 573 \\
&&&&&&&&&&&&&& 1 & 1 & 1 & 1 &&&&& = & 0 \\
6 & 1 &&& 1 & 1 &&& 1 & 1 &&& 1 & -3 && 3 & 3 && 6 &&&& = & 412 \\
6 & 1 & 1 &&& 1 & 1 &&& 1 & 1 &&& -3 &&& 3 & 3 && 6 &&& = & 408 \\
6 && 1 & 1 &&& 1 & 1 &&& 1 & 1 && -3 & 3 &&& 3 &&& 6 && = & 377 \\
&&&&&&&&&&&&&&&&&& 1 & 1 & 1 & 1 & = & 0
\end{array}\right\} \quad (181)$$

(The positions left empty contain zeroes, not shown.)

The entire set of relations may be put in matrix form, as in Equ. (181) and then various reduced situations may be extracted from it by the elimination of various lines, or equations, and the corresponding Columns. For computer calculation this may be done by simply changing the appropriate lines to equations such as $\hat{\delta}_1 = 0$.

For the more simple model of Treatments, without Carries-over, the variability residual on the control factors of Rows and Columns may be found most simply by the kind of calculations used in analysis of variance. If one actually works a case such as this not on a desk calculator there are two types of short-cut to the numerical results. For the situation without Treatments, i.e., the first line of the test of significance in Table XXXIIIb, since Rows and Columns are completely orthogonal one may use facilitating algebra of the sort that was used in the analysis of variance. The result is shown in Table XXXIIIb. With the introduction of treatment effects it is necessary to go to Equ. (181), less the lines involving δ_ℓ , because there is interaction between Rows and Treatments. These equations are very easily solved, even on a desk calculator, to give treatment estimates as shown in Table XXXIIIb. For the Rows, the estimates now become:

$$\left.\begin{array}{cccccc}
\hat{\alpha}_1 & \hat{\alpha}_2 & \hat{\alpha}_3 & \hat{\alpha}_4 & \hat{\alpha}_5 & \hat{\alpha}_6 \\
\hline
+11.39 & +12.58 & +3.50 & -8.81 & +9.39 & -18.75 \\
 & & & & & \\
\hat{\alpha}_7 & \hat{\alpha}_8 & \hat{\alpha}_9 & \hat{\alpha}_{10} & \hat{\alpha}_{11} & \hat{\alpha}_{12} \\
\hline
+4.17 & +6.86 & +1.72 & -11.08 & -10.17 & -.81
\end{array}\right\} \qquad (182)$$

When the Treatments become involved one may go, so long as the Design is perfect and regular, to Equ. (178) and (179) for precise, explicit estimates which can then be employed rapidly to yield row estimates. The estimates for Columns are, of course, quite independent of anything but $\hat{\mu}$. They are:

$\hat{\beta}_1$	$\hat{\beta}_2$	$\hat{\beta}_3$
-1.67	+1.00	+.67

(183)

For this model, the reduction in squares, all taken from the total sum of squares is

$$\begin{aligned} &159961 - \hat{\mu}2367 - \{\hat{\alpha}_1(231) + \hat{\alpha}_2(236) + \ldots + \hat{\alpha}_{12}(196)\} \\ &\qquad - \{\hat{\beta}_1(769) + \hat{\beta}_2(801) + \hat{\beta}_3(797)\} \\ &\qquad - \{\hat{\gamma}_1(610) + \hat{\gamma}_2(584) + \hat{\gamma}_3(573) + \hat{\gamma}_4(600)\} \\ &= 945.2222 \end{aligned} \tag{184}$$

when the estimates are calculated with many decimal places.

It might be supposed, and indeed was supposed when this experiment was done, that we might then proceed to consider the same data now with the possibility of Carry-over, included, as usual. As things turned out Equ. (181), in full, are singular. Such will appear by using the program in the Appendix. Accordingly, the analysis with Carry-over is impossible. The business of counting the number of observations and constants to be estimated in a repeated Design, such as the present, is a curious one.

To determine whether this is possible it is necessary to consider the basic Design--in the present case one of 4 Rows. Then one must count

$\hat{\mu}$	1	$\hat{\gamma}_k$	3
$\hat{\alpha}_i$	3	$\hat{\delta}_\ell$	3
$\hat{\beta}_j$	2		

for a total of 12 in 12 observations so that exact determination is possible. If the number of observations had fallen short the situation would be singular. It will be noted that the present Design,

Row I	(1)	(2)	(4)
Δ		1	2

proved singular; the repetition nowise affected the issue. It is not a matter of under-determination but a peculiar feature of the Design. This was unknown when the experiment was run. The Design

Row I	(1)	(3)	(4)
Δ		2	1

recommended in Table XII suffers from no such disability. The other Designs recommended in Chap. IV are all soluble, provided that they are not under-determined.

Table XXXIII - Satisfaction under four Treatments in three Periods, repeated thri

a. Design and Data

Group	Period 1	Period 2	Period 3	Sum
I	(1) 73	(2) 78	(4) 80	231
II	(2) 78	(3) 78	(1) 80	236
III	(3) 64	(4) 71	(2) 71	206
IV	(4) 61	(1) 65	(3) 46	172
V	(1) 89	(2) 67	(4) 69	225
VI	(2) 45	(3) 53	(1) 44	142
VII	(3) 70	(4) 64	(2) 74	208
VIII	(4) 64	(1) 77	(3) 78	219
IX	(1) 59	(2) 71	(4) 72	202
X	(2) 49	(3) 56	(1) 60	165
XI	(3) 56	(4) 58	(2) 51	165
XII	(4) 61	(1) 63	(3) 72	196
Sum	769	801	797	2367
Sum sq.	50,911	54,267	54,783	

b. Analysis for direct Treatment alone

	$\widehat{(1)}$	$\widehat{(2)}$	$\widehat{(3)}$	$\widehat{(4)}$
Contr.	+1.75	-1.17	+.42	-1.00
Adj. Mn.	67.50	64.58	66.17	64.75

Factors	d.f	Residual Variability (Squares)
$\hat{\mu}$. Rows & Columns (control)	22	990.0000
Control factors plus Treatment	19	945.2222

$$F_{3,19} = \frac{(990.0000 - 945.2222)/3}{945.2222/19} = .30 \text{ N.S.}$$

Analysis with both direct Treatment and Carry-over is impossible.

It might reasonably be objected that the Design of Table XXXIII leaves something to be desired, since the total pattern seems over-repeated. It is, however, impossible to do much better. One cannot write in sets of 4 Rows with cyclic Columns and get, say, Treatment (1) followed equally by all three other Treatments.

Missing data situations - As was previously observed in connection with latin squares and single Youdens in many fields of actual practice one is bedevilled by incomplete Designs. Perhaps, as in the situation reported in Table XXXIV, one has 7 Treatments to try and it is practical to try 4 on each of a number of men. Hence there is indicated a paired Youden, 2(7x4x7), as in Table XVII . In these,it should be remembered, the Carry-over is balanced, each Treatment if preceded by every other Treatment. There was, however, the practical difficulty that it was necessary to use 20 existing Groups of participants for this Design that requires 14 Rows. As can be seen from the data, the Design was laid out for the first 14 Rows and then the first 6 lines of the Design were repeated. Since, however, each Row is associated with a Man, and men tend to disappoint us, we suffer then losses. As can be seen from the interruption of the cyclic arrangement of Treatments in Columns, the 3rd and 19th Men are missing (in the table they have been renumbered). As it happens the losses were such that all lines of the original Design are represented once although 4 are still represented twice. It could easily have happened that some were represented not at all. All this is above the regular problem of the literature which conceives a perfect and complete Design except that an observation is missing here or there. As has been previously remarked, the problems, or the consequences of Design incomplete by whole Rows are much more serious for Youden rectangles, when $c < t$, than for latin squares, when $c = t$. This is because in the rectangles Treatment and Row are confounded. In the example of Table XXXIV, certain comparisons, within Rows, occur 6 times, certain 5 times and others but 4 times. Likewise, a given Treatment follows some Treatments twice but other Treatments only once. Finally, some Treatments occur thrice in a Column over others but twice. Such shortcomings, obviously, make any simple explicit solution of any kind quite impossible but do not at all seriously handicap the comparison of Treatments. These data are put quite simply into the program, as in Appendix for electronic computer with the results shown in Table XXXIVb or Table XXXIVc.

It is possible to set up for Table XXXIV a full set of equations embracing $\hat{\mu}$, $\hat{\alpha}_i$, $\hat{\gamma}_k$ and $\hat{\delta}_\ell$ in matrix form. This would be somewhat tedious because of the size of the matrix. Among other things it is blown up by the number of Rows. Granted that in principle, it could be set up then one would go to work solving for models of various degrees of complexity, as previously, and generate estimates of effects of Treatment and Carry-over, respectively, and corresponding tests of significance.

The first thing that may be noted in connection with the least squares equations is that since the Rows and Columns remain orthogonal the variability residual on their estimates (at the lowest level) can still be gotten by the operations of the type associated with analysis of variance. The second thing is that since various Treatments and Carries-over occur various numbers of times, the estimate of $\hat{\mu}$ comes from

$$72\hat{\mu} - \hat{\gamma}_4 - \hat{\gamma}_6 + \hat{\gamma}_7 + \hat{\delta}_2 - \hat{\delta}_6 + \hat{\delta}_7 = 3277 \tag{185}$$

The Row estimates are confounded to just the extent they would be if the Design were perfect, i.e., we have

$$\left.\begin{array}{l} 4\hat{\alpha}_1 - \hat{\gamma}_3 - \hat{\gamma}_5 - \hat{\gamma}_6 + \hat{\delta}_1 + \hat{\delta}_2 + \hat{\delta}_4 + 4\hat{\mu} = 184 \\ 4\hat{\alpha}_2 - \hat{\gamma}_4 - \hat{\gamma}_6 - \hat{\gamma}_7 + \hat{\delta}_2 + \hat{\delta}_3 + \hat{\delta}_5 + 4\hat{\mu} = 125 \\ \cdot\;\cdot\;\cdot\;\cdot\;\cdot\;\cdot\;\cdot\;\cdot\;\cdot\;\cdot\;\cdot\;\cdot\;\cdot\;\cdot\;\cdot\;\cdot\;\cdot\;\cdot\;\cdot\;\cdot\;\cdot\;\cdot\;\cdot\;\cdot\;\cdot \\ 4\hat{\alpha}_{20} - \hat{\gamma}_1 - \hat{\gamma}_3 - \hat{\gamma}_4 + \hat{\delta}_6 + \hat{\delta}_7 + \hat{\delta}_2 + 4\hat{\mu} = 205 \end{array}\right\} \tag{186}$$

The column estimates are no longer orthogonal to everything but are:

$$\left.\begin{array}{r} 18\hat{\beta}_1 - \hat{\gamma}_3 - \hat{\gamma}_5 + 18\hat{\mu} = 789 \\ 18\hat{\beta}_2 - \hat{\gamma}_4 - \hat{\gamma}_6 - \hat{\delta}_3 - \hat{\delta}_5 + 18\hat{\mu} = 821 \\ 18\hat{\beta}_3 - \hat{\gamma}_6 - \hat{\gamma}_1 - \hat{\delta}_4 - \hat{\delta}_6 + 18\hat{\mu} = 794 \\ 18\hat{\beta}_4 - \hat{\gamma}_2 - \hat{\gamma}_4 - \hat{\delta}_6 - \hat{\delta}_1 + 18\hat{\mu} = 873 \end{array}\right\} \tag{187}$$

The treatment estimates are confounded because $c < t$ and further by the irregularity of Rows to become hideous as

$$10\hat{\gamma}_1 - \hat{\alpha}_3 - \hat{\alpha}_5 - \hat{\alpha}_8 - \hat{\alpha}_9 - \hat{\alpha}_{11} - \hat{\alpha}_{16} - \hat{\alpha}_{17} - \hat{\alpha}_{18} + \hat{\beta}_1 + \hat{\beta}_4 - \hat{\delta}_1 + \hat{\delta}_5 + 10\hat{\mu} = 421 \tag{188}$$

etc.

The least squares equations for Carries-over are likewise confounded.

In spite of the complexity of the confounding, which much influences the business of estimating effects, the calculation of reduced variability then remains as simple as ever. For $\hat{\mu}$, Rows and Columns, which are still unconfounded , appeal may be made either to the technique of analysis of variance or to orthogonal least squares equations. For the model, without Carry-over, the estimates of treatment effects are as in Table XXXIVb and other estimates are:

$$\left.\begin{array}{cccc} \hat{\mu} = 45.50 & & & \\ \alpha_1 & \alpha_2 & \cdots\cdots\cdots & \alpha_{18} \\ \hline +1.25 & -14.52 & \cdots\cdots\cdots & +6.13 \\ & & & \\ \hat{\beta}_1 & \hat{\beta}_2 & \hat{\beta}_3 & \hat{\beta}_4 \\ \hline -1.39 & -.10 & -1.34 & +2.83 \end{array}\right\} \qquad (189)$$

so that the estimate of residual squares is simply

$$\begin{aligned} & 155{,}985 - 45.50(3277) \\ & \quad - \{+ 1.25(184) - 14.52(125) + \ldots\ldots. + 6.13(205)\} \\ & \quad - \{- 1.39(789) - .10(821) - 1.34(794) + 2.83(873)\} \\ & \quad - \{- 1.99(421) - 1.18(480) + \ldots\ldots\ldots + .78(535)\} \\ & \quad = 1973.2000 \end{aligned} \qquad (190)$$

if the estimates, with many decimal places, are in fact employed. This result is shown in Table XXXIVb.

For the model without Treatments, the estimates are:

$$\left.\begin{array}{cccc} \multicolumn{4}{l}{\hat{\mu} = 45.46} \\ \hat{\alpha}_1 & \hat{\alpha}_2 & \ldots\ldots\ldots & \hat{\alpha}_{18} \\ \hline -.61 & -14.24 & \ldots\ldots & +5.28 \\ \hat{\beta}_1 & \hat{\beta}_2 & \hat{\beta}_3 & \hat{\beta}_4 \\ \hline -1.63 & -.07 & -1.32 & +3.02 \\ \hat{\delta}_1 & \hat{\delta}_2 & \ldots\ldots\ldots & \hat{\delta}_7 \\ \hline +1.07 & +3.35 & \ldots\ldots & -.66 \end{array}\right\} \qquad (191)$$

The resultant residual variability is gotten on the lines of Equ. (190). In a similar way for the full model the estimates of treatment effects and Carries-over are as shown in Table XXXIVc and other estimates are:

$$\left.\begin{array}{cccc} \multicolumn{4}{l}{\hat{\mu} = 45.45} \\ \hat{\alpha}_1 & \hat{\alpha}_2 & \ldots\ldots\ldots & \hat{\alpha}_{18} \\ \hline +.12 & -14.90 & \ldots\ldots & +5.97 \\ \hat{\beta}_1 & \hat{\beta}_2 & \hat{\beta}_3 & \hat{\beta}_4 \\ \hline -1.37 & -.17 & -1.34 & +2.88 \end{array}\right\} \qquad (192)$$

so that the estimate of residual squares is

$$\begin{aligned} & 155{,}985 - 45.45(3277) \\ & \quad - \{+ .12(184) - 14.90(125) + \ldots + 5.97(205)\} \\ & \quad - \{- 1.37(789) - .17(821) - 1.34(794) + 2.88(873)\} \\ & \quad - \{- 1.41(421) - .53(480) + \ldots + 1.19(535)\} \\ & \quad - \{+ .74(321) + 2.84(453) + \ldots - .64(391)\} \\ & \quad = 1874.1917 \qquad (193) \end{aligned}$$

if the estimates are employed, in fact, with many decimal places. This result is shown in Table XXXIVc.

Table XXXIV - An example of the highly incomplete kind of Design that occurs in practice. A partially repeated paired Youden, 2(7x4x7).

a. Design and data

Man	Week 1	Week 2	Week 3	Week 4	Sum
I	(1) 55	(2) 37	(4) 47	(7) 45	184
II	(2) 37	(3) 25	(5) 25	(1) 38	125
III	(4) 24	(5) 24	(7) 29	(3) 46	123
IV	(5) 36	(6) 44	(1) 39	(4) 52	171
V	(6) 42	(7) 54	(2) 50	(5) 55	201
VI	(7) 49	(1) 37	(3) 56	(6) 62	204
VII	(1) 42	(5) 45	(3) 35	(2) 32	154
VIII	(2) 55	(6) 60	(4) 52	(3) 60	227
IX	(3) 54	(7) 60	(5) 57	(4) 54	225
X	(4) 42	(1) 44	(6) 46	(5) 44	176
XI	(5) 46	(2) 48	(7) 54	(6) 47	195
XII	(6) 42	(3) 54	(1) 42	(7) 42	180
XIII	(7) 49	(4) 42	(2) 47	(1) 48	186
XIV	(1) 34	(2) 43	(4) 46	(7) 54	177
XV	(2) 52	(3) 55	(5) 42	(1) 42	191
XVI	(3) 38	(4) 38	(6) 26	(2) 28	130
XVII	(4) 50	(5) 62	(7) 50	(3) 61	223
XVIII	(6) 42	(7) 49	(2) 51	(5) 63	205
Sum	789	821	794	873	3277
Sum sq.	35,749	39,499	36,672	44,065	

b. Analysis for direct Treatment, without allowance for Carry-over

	$(\hat{1})$	$(\hat{2})$	$(\hat{3})$	$(\hat{4})$	$(\hat{5})$	$(\hat{6})$	$(\hat{7})$
Contr.	-1.99	-1.18	+4.13	-.60	+.14	-1.28	+.78
Adj.Mn.	43.51	44.32	49.63	44.90	45.64	44.22	46.28

Factors	d.f.	Residual Variability (Squares)
$\hat{\mu}$, Rows & Columns (control)	51	2188.3194
Control factors plus Treatment	45	1973.2000

$$F_{6,45} = \frac{(2188.3194 - 1973.2000)/6}{1973.2000/45} = .82 \text{ N.S.}$$

c. Analysis for direct Treatment and Carry-over

	$(\hat{1})$	$(\hat{2})$	$(\hat{3})$	$(\hat{4})$	$(\hat{5})$	$(\hat{6})$	$(\hat{7})$
Contr.	-1.41	-.53	+3.83	-.98	-.60	-1.49	+1.19
Adj.Mn.	44.04	44.92	49.25	44.47	44.85	43.96	46.64

	$[\hat{1}]$	$[\hat{2}]$	$[\hat{3}]$	$[\hat{4}]$	$[\hat{5}]$	$[\hat{6}]$	$[\hat{7}]$
Contr.	+.74	+2.84	+.71	-.14	-2.04	-1.46	-.64

Factors	d.f.	Residual Variability (Squares)
$\hat{\mu}$, Rows, Columns & Carries-over (control)	45	2045.6489
Control factors plus Treatments	39	1874.1917

$$F_{6,39} = \frac{(2045.6489 - 1874.1917)/6}{1874.1917/39} = .59 \text{ N.S.}$$

In Table XXXIVc, not only are the effects of direct Treatment $(\hat{k})$, and Carry-over $[\hat{\ell}]$, of low significance but their correspondence, i.e., their correlation is only +.16.

To conclude the discussion on situations such as that in Table XXXIV, where essentially Rows are missing, two things may be observed. First, it becomes essential to analyze by use of least squares equations, for even μ and Columns are, of course, no longer orthogonal to other effects and must be embraced in the general system of simultaneous equations. The degrees of freedom are a little reduced for Rows and for residual variability.

Designs more or less Youden - From the general preceding discussion and from the case of Table XXXIV, in particular, it should have become fairly plain that with the best of intentions, in a practical way all we get is a Design that is more or less Youden and its shortcomings ma[illegible] little practical difference. This must lead us to wonder whether w[illegible] might not be a lot more casual about designing and indeed we may. If we use, as we generally must, either the method of writing out least squares equations or a program that does the same thing directly from Design and data, the balanced and nice features of the Youden are no longer vital. For cases with $c < t$, i.e., the rectangles, we are essentially in a situation where all cells of the combination of Rows and Treatment are not filled--we are in a missing plot situation. We carefully arrange this incomplete fill so that solutions for treatment effects in terms of sums of observations are fairly simple. Thus arise all our niceties. Freed from the limitations of the desk calculator we should rewrite our statistical method--as has been observed by a number of people in recent years. The present situation is an illustration of the general principle.

There remain some limitations to the writing of Designs that have varying strictness. It is assumed that the Designs that might be written would be in Rows and Columns as at present, that we should be fundamentally interested in comparisons within Rows, i.e., that Rows would tend to have less variability within than among, and that Carry-over would remain a possibility. Some of the limitations would then be:

a. Probably there should be as many trials projected for one subject or individual as another, i.e., the Rows should be of equal length. Then we might continue to write in terms of t Treatments in c Columns for r Rows.

b. The number cr of observations would have to be great enough to permit explicit solution for all the effects, Row, Column, Treatment and Carry-over that would be required.

c. There would probably be little point to a Design where the fill, $f = c(c - 1)/(t - 1)$, were less than unity, i.e., it were not possible to contrast each Treatment with each other Treatment at least once.

d. While we may be abandoning the concept that f need be an integer, i.e., that all comparisons can be made the same number of times, it probably remains desirable that all comparisons be made more or less the same number of times. Otherwise the tests of significance may become badly out.

e. It remains imperative that all Treatments are interknit. Thus it would not do for Treatments (1) through (6) to be written exclusively in certain Rows and (7) through (15) to be written exclusively in other Rows with no Rows containing some of both sets of Treatments. Nor may one even approach such a condition.

f. So nearly as possible, all Treatments should occur equally in all Columns.

Such Designs belong, of course, to the class of partially balanced, as in the literature.

As an illustration of a Design not Youden, but falling well within the limitations first set out, one might write half a double Youden (9x4x18) as in Table XXXV, taken from Table XXXI. In Table XXXV half the comparisons occur once and half twice. Perhaps in a more general way, we could conceive a Design where all comparisons occur at least once but some once more than others. The results from such a Design can be put through the analysis by least squares equations, such as we have used for strict Youden Designs, quite easily with the results shown in Table XXXV. (It must be allowed that this particular experiment is rather meager.)

The complete system of relations obtaining from the results of Table XXXV may be written out by noting that

$$36\hat{\mu} = 93.6 \tag{194}$$

$$\left.\begin{array}{l} 9\hat{\beta}_1 + 9\hat{\mu} = 23.6 \\ \cdots\cdots\cdots\cdots\cdots \\ 9\hat{\beta}_4 + 9\hat{\mu} = 24.0 \end{array}\right\} \tag{195}$$

and the matrix, regular like that for the full, perfect, simple Youden rectangles, previously shown, as follows:

$$\left.\begin{array}{ccccccccccccccccccccccccccccccr}
\hat{\mu} & \hat{\alpha}_1 & \hat{\alpha}_2 & \hat{\alpha}_3 & \hat{\alpha}_4 & \hat{\alpha}_5 & \hat{\alpha}_6 & \hat{\alpha}_7 & \hat{\alpha}_8 & \hat{\alpha}_9 & \hat{\beta}_1 & \hat{\gamma}_1 & \hat{\gamma}_2 & \hat{\gamma}_3 & \hat{\gamma}_4 & \hat{\gamma}_5 & \hat{\gamma}_6 & \hat{\gamma}_7 & \hat{\gamma}_8 & \hat{\gamma}_9 & \hat{\delta}_1 & \hat{\delta}_2 & \hat{\delta}_3 & \hat{\delta}_4 & \hat{\delta}_5 & \hat{\delta}_6 & \hat{\delta}_7 & \hat{\delta}_8 & \hat{\delta}_9 & = & \text{Sum} \\
\hline
36 & &&&&&&&& & & &&&&&&&& & &&&&&&&& & = & 93.6 \\
4 & 4&&&&&&&& & & 1&1&&1&&&&1& & 1&1&&1&&&&& & = & 12.5 \\
4 & &4&&&&&&& & & &1&1&&1&&&1 & &1&1&&1&&&& & = & 10.7 \\
4 & &&4&&&&&& & & 1&&1&1&&1&&& & &&1&1&&1&&& & = & 10.1 \\
4 & &&&4&&&&& & & &1&&1&1&&1&& & &&&1&1&&1&& & = & 8.1 \\
4 & &&&&4&&&& & & &&1&&1&1&&1& & &&&&1&1&&1& & = & 9.3 \\
4 & &&&&&4&&& & & &&&1&&1&1&&1 & &&&&&1&1&&1 & = & 11.8 \\
4 & &&&&&&4&& & & 1&&&&1&&1&1& & 1&&&&&&1&1& & = & 9.5 \\
4 & &&&&&&&4& & & &1&&&&1&&1&1 & &1&&&&&&1&1 & = & 13.8 \\
 & 1&1&1&&&&&1&1 & & &&&&&&&& & &&&&&&&& & = & .0 \\
9 & &&&&&&&& & 9 & &&&&&&&& & &&&&&&&& & = & 23.5 \\
4 & 1&&1&&&&1&&1 & & 4&&&&&&&& & &&&&&1&&1&1 & = & 8.3 \\
4 & 1&1&&1&&&&1& & & &4&&&&&&& & 1&&&&&&1&&1 & = & 11.6 \\
4 & &1&1&&1&&&&1 & & &&4&&&&&& & 1&1&&&&&&1& & = & 8.5 \\
4 & 1&&1&1&&1&&& & & &&&4&&&&& & &1&1&&&&&&1 & = & 12.0 \\
4 & &1&&1&1&&1&& & & &&&&4&&&& & 1&&1&1&&&&& & = & 8.8 \\
4 & &&1&&1&1&&1& & & &&&&&4&&& & &1&&1&1&&&& & = & 12.3 \\
4 & &&&1&&1&1&&1 & & &&&&&&4&& & &&1&&1&1&&& & = & 7.8 \\
4 & 1&&&&1&&1&1& & & &&&&&&&4& & &&&1&&1&1&& & = & 12.0 \\
 & &&&&&&&& & & 1&1&1&1&1&1&1&1&1 & &&&&&&&& & = & .0 \\
3 & 1&&&&&&1&&1 & -1 & &1&1&&1&&&& & 3&&&&&&&& & = & 7.6 \\
3 & 1&1&&&&&&1& & -1 & &&1&1&&1&&& & &3&&&&&&& & = & 9.7 \\
3 & &1&1&&&&&&1 & -1 & &&&1&1&&1&& & &&3&&&&&& & = & 7.3 \\
3 & 1&&1&1&&&&& & -1 & &&&&1&1&&1& & &&&3&&&&& & = & 7.6 \\
3 & &1&&1&1&&&& & -1 & &&&&&1&1&&1 & &&&&3&&&& & = & 7.6 \\
3 & &&1&&1&1&&& & -1 & 1&&&&&&1&1& & &&&&&3&&& & = & 6.6 \\
3 & &&&1&&1&1&& & -1 & &1&&&&&&1&1 & &&&&&&3&& & = & 8.5 \\
3 & &&&&1&&1&1& & -1 & 1&&1&&&&&&1 & &&&&&&&3& & = & 7.0 \\
 & &&&&&&&& & & &&&&&&&& & 1&1&1&1&1&1&1&1&1 & = & .0
\end{array}\right\} \quad (196)$$

(The positions, left empty, contain zeroes not shown)

Proceeding with the model at various levels from Equ. (196), or in practice use the program in the Appendix, there are gotten the results of Table XXXV.

Table XXXV - An example of near-Youden Design

a. Design and Data

	Period				
Man	1	2	3	4	Sum
I	(1) 2.5	(2) 3.5	(4) 3.4	(8) 3.1	12.5
II	(2) 2.3	(3) 2.5	(5) 2.5	(9) 3.4	10.7
III	(3) 2.2	(4) 3.1	(6) 2.7	(1) 2.1	10.1
IV	(4) 2.2	(5) 1.8	(7) 1.6	(2) 2.5	8.1
V	(5) 2.3	(6) 2.6	(8) 2.5	(3) 1.9	9.3
VI	(6) 3.2	(7) 2.0	(9) 3.3	(4) 3.3	11.8
VII	(7) 2.5	(8) 2.7	(1) 2.1	(5) 2.2	9.5
VIII	(8) 3.7	(9) 3.0	(2) 3.3	(6) 3.8	13.8
IX	(9) 2.6	(1) 1.6	(3) 1.9	(7) 1.7	7.8
Sum	23.5	22.8	23.3	24.0	93.6

b. Comparisons

vs.	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
(1)		o	oo	oo	o	o	oo	oo	o
(2)	x		o	oo	oo	o	o	oo	oo
(3)	xx	x		o	oo	oo	o	o	oo
(4)	xx	xx	x		o	oo	oo	o	o
(5)	x	xx	xx	x		o	oo	oo	o
(6)	x	x	xx	xx	x		o	oo	oo
(7)	xx	x	x	xx	xx	x		o	oo
(8)	xx	xx	x	x	xx	xx	x		o
(9)	x	xx	xx	x	x	xx	xx	x	

c. Analysis for direct Treatment alone

	$(\hat{1})$	$(\hat{2})$	$(\hat{3})$	$(\hat{4})$	$(\hat{5})$	$(\hat{6})$	$(\hat{7})$	$(\hat{8})$	$(\hat{9})$
Contr.	-.53	+.16	-.30	+.39	-.22	+.37	-.46	+.21	+.37
Adj. Mn.	2.07	2.76	2.30	2.99	2.38	2.97	2.14	2.81	2.97

Factors	d.f.	Residual Variability (Squares)
$\hat{\mu}$, Rows & Columns (control)	24	5.13
Control factors plus Treatments	16	1.53

$$F_{8,16} = \frac{(5.13 - 1.53)/8}{1.53/16} = 4.71^{**}$$

d. Direct treatment and carry-over estimates from simultaneous equations

	$(\hat{1})$	$(\hat{2})$	$(\hat{3})$	$(\hat{4})$	$(\hat{5})$	$(\hat{6})$	$(\hat{7})$	$(\hat{8})$	$(\hat{9})$
Contr.	-.14	+.29	-.27	+.07	-.51	+.07	-.56	+.35	+.70
Adj. Mn.	2.46	2.89	2.33	2.67	2.09	2.67	2.04	2.95	3.30

	$[\hat{1}]$	$[\hat{2}]$	$[\hat{3}]$	$[\hat{4}]$	$[\hat{5}]$	$[\hat{6}]$	$[\hat{7}]$	$[\hat{8}]$	$[\hat{9}]$
Contr.	+.25	+.53	+.52	+.03	+.04	-.45	-.31	-.61	-.00

Factors	d.f.	Residual Variability (Squares)
$\hat{\mu}$, Rows, Columns & Carries-over (control)	16	3.23
Control factors plus Treatments	8	.82

$$F_{8,8} = \frac{(3.23 - .82)/8}{.82/8} = 2.95 \text{ N.S.}$$

Factors	d.f.	Residual Variability (Squares)
$\hat{\mu}$, Rows, Columns & Treatments (control)	16	1.53
Control factors plus Carries-over	8	.82

$$F_{8,8} = \frac{(1.53 - .82)/8}{.82/8} = .87 \text{ N.S.}$$

The results of Table XXXV are a little difficult to interpret in a practical way. Briefly, the effect of Treatments is significant when Carry-over is not involved but fails significance when it is. Carry-over, per se, is far from significant. Finally, the correlation between Treatment and Carry-over is the trivial -.08. In such a practical situation, an experimentalist would probably be inclined to take his decision on the more simple model. There remain, of course, problems of a fine theoretical kind; if one always chooses as it were the better or best result, then all he need do is try enough models and something will be trivially significant. In the present case, it happens that the results may be compared with those from the fuller Table XXXI. Then it will be noted that, again, the inclusion of Carry-over reduced the value of F for Treatments. Further reflection gives the further impression that the estimates for Treatments are much the more stable, from the one table to the other, in the situation where Carry-over is not involved.

Breaking Treatment variability up - Very often, as was discussed previously in connection with the latin square and the single Youden, after data have been subject to analysis for the general significance of Treatments the variability among Treatments must be further broken up. The Treatments are not simply so many alternate varieties, in themselves. Procedure of this type is further illustrated on a (double) Youden rectangle, where $c < t$. An example of an experiment, where it is necessary to separate out component factors among the Treatments is provided there by the data of Table XXXI, on a 9x4x18. The results will be discussed now from the point of view of breaking down the variability of Treatments into component factors. There they were regarded simply as 9 Treatments. The Treatments consist of 3 chemical or physical variables each at 2 levels (a high value and a low value), in all combinations to yield, in the symbolism usual in this field:

High A, high B, high C = abc
High A, high B, low C = ab
High A, low B, high C = ac
High A, low B, low C = a
Low A, high B, high C = bc
Low A, high B, low C = b
Low A, low B, high C = c
Low A, low B, low C = (8)
Standard sample.

In such a problem it is necessary to sort out the orthogonal effects of A, B and C and their interactions. The neophyte may wonder, indeed, at the inclusion of Treatment (9) but will find that his "practical" colleague insists on including a "bench-mark."

In partitioning the sums of squares in a Youden setup, the best procedure seems to be to work with the treatment estimates very much as in the more simple problems one works with totals and then finally to adjust sum of residual squares in allowance for scale. This is the general procedure, recommended and followed for the earlier illustrations. If our business is to partition the effect of Treatments without allowance for Carry-over, we may start with the estimates in Table XXXIb. Our first business is to separate out the odd comparison of (9), the "bench-mark" from the experimental comparisons. Following Equ. (36)

$$K = \frac{9.0988 - 2.8211}{(-.48)^2+(+.18)^2+(-.43)^2+(+.17)^2+(-.09)^2+(+.36)^2+(-.33)^2+(+.23)^2+(+.40)^2}$$

$$= 6.7552 \qquad (197)$$

(when actually estimates with 4 decimal places are used in the denominator). On the model of Equ. (35), the variability among the 9 Treatments that is due to the single comparison of this standard with experimental material is

$$6.7552 \{(+.40)^2 + (-.48 +.18 -.43 +.17 -.09 \quad +.36 -.33 +.23)^2/8\}$$

$$= 6.7552 \{(+.40)^2 + (-.39)^2/8\}$$

$$= 1.1935 \; . \qquad (198)$$

The variability between the two levels of A required as usual, the total for all results under the high level A or, as it is commonly put, the comparison

$$(abc + ab + ac + a) - (bc + b + c + (8)) .$$

This in the terms of the Design of Table XXXI involves the comparison of the total for (1), (2), (3) and (4) with the total for all results under the low level, i.e., (5), (6), (7) and (8) and also the total under all 8 of these Treatments. The result from the general Equ. (35) is

$$6.7552 \{ \frac{(-.56)^2}{4} + \frac{(+.17)^2}{4} - \frac{(-.39)^2}{8} \}$$

$$= .4495 \qquad (199)$$

Similarly the variability between the 2 levels of B is, as it is commonly put, due to the comparison

$$(abc + ab + bc + b) - (as + a + c + (8))$$

or in the terms of Table XXXI of (1), (2), (5) and (6) with (3), (4), (7) and (8). The result is

$$6.7552 \{ \frac{(-.03)^2}{4} + \frac{(-.36)^2}{4} - \frac{(.39)^2}{8} \}$$

$$= .0959 \qquad (200)$$

Similarly the variability between the 2 levels of C is

$$6.7552 \{ \frac{(-1.33)^2}{4} + \frac{(+.94)^2}{4} - \frac{(.39)^2}{8} \}$$

$$= 4.3242 . \qquad (201)$$

There remains some variability among Treatments ascribable to interactions of the factors A, B and C. It is easily enough gotten out, if of interest, but it is somewhat beyond our present business to go too fully into the analysis of variance in this sense. To those familiar with the general procedures there will be no problem. The remainder mean square is gotten by

$$(9.0988 - 2.8211 - 1.1935 - .4495 - .0959 - 4.3242)/4 = .0536 . \qquad (202)$$

Finally, all these mean squares must be compared with residual mean square of

$$2.81143/43 = .0654 . \qquad (203)$$

The whole business may be conveniently packaged or presented in the form of the analysis of sums of squares as shown in Table . XXXVIa.

One can, alternatively, partition the treatment effects after allowance had been made for Carry-over. These calculations would be based upon Table XXXIc instead of XXXIb. The main difference would be in the reduced number of degrees of freedom for residual factors. The results are summarized in Table XXXVIb. One might even partition Carry-over, although in this table it is so slight as hardly to merit such delicacy.

It may be noted that the present breakdown is completely correct because all the Treatments are represented equally in this Design. We are not in the position, as with incomplete Designs, of making some kind of apology for the discussion being a little approximate. It may be noted that where such balance does not exist, the partitioning of variability among Treatments may lead to some highly anomalous results.

Table XXXVI - Analysis of sums of squares and of mean squares in breaking up treatment effects

a. Separation of treatment effects without allowance for Carry-over

Source	d.f.	Mean Squares	F
Factor A	1	.4495	6.87 *
Factor B	1	.0959	1.47 N.S.
Factor C	1	4.3242	66.12 ***
Exp. Remainder	4	.0536	.82 N.S.
Stan. vs. Exp.	1	1.1935	18.25 ***
Residual	37	.0654	

b. Separation of Treatment effects with allowance for Carry-over

Source	d.f.	Mean Squares	F
Factor A	1	.3475	5.33 *
Factor B	1	.1195	1.83 N.S.
Factor C	1	2.7760	42.58 ***
Exp. Remainder	4	.0558	.86 N.S.
Stan. vs. Exp.	1	1.1606	17.80 ***
Residual	29	.0652	

From the analysis as of Table XXXVIa, factor A has a significant (5% level) effect. Also factor C has a significant (.1% level) effect. It is important not to rest at this point, perhaps from the exhaustion of getting out the significances but to remember that the world wants to know at what level of A and C it should operate. This can, of course, be found by referring to the estimates of Table XXXIb. Such reference to the estimates of Treatment effects shows that the higher value of A is disfavored--by an amount of, say, $(-.56 + .17)/4 = -.18$. On the other hand, the higher value of C is favored--by an amount of, say, $(-1.33 + .94)/4 = +.57$. The interactions of A, B and C, i.e., the remainder term, were not significant. The importance of this operation is that we may thus discover the highly significant elements among the Treatments as a whole.

The way one handles results from an experiment depends, of course, on the fundamental nature of the practical problem involved. Estimates, such as those discussed, of treatment effects, are in many experiments, all that one needs from the Youden analysis. More commonly one goes on to some test of the significance of the variation among the estimates. Very often one stops here, saying they are just t things, they do vary significantly and such and such are best. It may, at times, be of practical interest to go on and regress estimates of result against some measurable character of the Treatments. In other studies the treatment estimates may be displayed against the background of factors at various levels and in combination, as has just been done in Table XXXVI. The variety of problems and of procedure is endless. To discuss them all fully would take a much greater book than the present; it would take indeed a very general textbook of statistics.

Design with several sections - In actual experimentation one may always encounter problems where an experiment is done in several sections. This is true of change-over experiments among others. Thus one may have, for a very simple case, a 7x4x14 experiment carried out in 2 hospitals, for instance, essentially the first Design of Table XVII except that the first 7 Rows are done in the one and the last 7 in the other. Necessarily, there are 2 physicians and one may work in the spring and the other in the autumn. Now there obviously arise a number of serious questions such as whether the effects of Columns are the same in the 2 sections. It may be that the estimates for Columns tend to rise with the passage of time, i.e., progress through the table of data in the first hospital but fall in the second. Such effects often occur with season. Then plainly, one cannot necessarily take out a column effect common to the two hospitals. One will have to do something like analyze them separately, each for its own kind of column effect and then make some kind of combination of treatment effects. The possibilities of difficulty are endless. One may have to anticipate that there is actually a difference in treatment effects between hospitals. One may even have to anticipate that the magnitude of extraneous variability is very different from one hospital to the other, as is all too often the case and as becomes very serious when the two sections are of different size; one is a 7x4x14 and the other a 7x4x7. The problems multiply when there are more than 2 hospitals.

The type of difficulty envisaged is common in Agriculture where sections of the Design are run in several successive years. Then the effect of time, the very effect of experimental Treatment and the nature of extraneous variability may be anticipated to vary.

The difficulties of an experiment, in several sections, are nowise peculiar to change-over experiments. An adequate discussion of them would require a work much beyond the scope of the present book. Here it is impossible to equip people to deal adequately with such a diversity of problems; they would have to acquaint themselves with the much larger and more general problems. Here it can only be said that such problems are quite natural and real and the analysis of such Designs should be approached with the greatest caution.

VIII. Designs involving pairs or a single Change-over

Page

The peculiar nature of studies with single Change-over 284

Design with single Change-over 286

Analysis when there is no conditioning Period in the Design . . . 291

Analysis when there is a conditioning Period in the Design . . . 301

Procedure when there are 2 Treatments 307

Analysis when there are 3 Treatments 327

Breaking up treatment variability 328

Missing data situations on paired Designs 329

Double reversal trials 333

The peculiar nature of studies with single Change-over - Designs involving a single Change-over, i.e., the study of only 2 Treatments or Materials on one experimental unit, where quantity is involved are, as said at the very beginning of the present book, probably the most fundamental form of experimentation. They are the form of design naturally set up by men, naive about such things. The instincts of these men are very sound. They often unfortunately suppose, however, that since things can only be compared in pairs, then it is only possible to study two things. In fact, by the combination of many comparisons it is possible and often very profitable to study a number of Materials or Treatments with paired comparisons. We might, for instance, study 4 Treatments, (1), (2), (3) and (4) by issuing the pair (1) and (2) to one man, (1) and (3) to another man and so on for (1) and (4), (2) and (3), (2) and (4), (3) and (4) in all to 6 men. As was discussed in Chap. I, various rearrangements and extensions might be made to balance firstness and secondness etc. Each man would yield some sort of rating to each member of the pair. It should be reiterated that the present discussion is concerned with quantity. Note that there is another class of paired comparisons where it is, so-to-speak, the games won rather than the number of goals that tell. Such data would arise if it were a matter of deciding which were the better Treatment for a patient. This is, of course, a most interesting subject on which much has and could be written but not here. The question of preferences is outside the realm of the present discussion. Nor, for that matter, are we concerned with intimate "simultaneous" quantitative estimations such as the famous technique in Animal Husbandry, of trying two diets on twin calves. With such data we are not here concerned but rather with quantitative results such as number of hours slept or at least quantitative ratings.

Biometricians involved in paired comparison considerations seem to worry a lot about circularities or eddies of preference. Thus possibly (1) beats (2), (2) beats (3), (3) beats (1). Thus a man can win against his wife at chess, she can win over her mother but the latter can win over the son-in-law. The moral superiorities are in three different dimensions. No doubt similar complexities could occur if several things were tried by one participant as when he might try 3 or 4 things in succession. The possibility of such complexity does not seem to bother people. It would be so complex that it would be hard to see and therefore not the occasion of concern. "What the eye does not see the heart does not grieve."

Just as in Youdens in general or for latin squares, as a special case, so in paired studies Carry-over of treatment effects may often be considerable and should be allowed for if possible. It may, however, be necessary to free the estimates of treatment effects from Carry-over. It may further be required to get Carry-over out of the residual mean square--Carry-over blows it up. There are even times when it is of great practical importance to estimate the effect of Change-over. Thus we are often under the necessity of analyzing for Change-over. Indeed, in paired comparisons, the problem of Carry-over may be more acute than in any other class of design. This is so because the confounding of Carry-over with direct treatment effect may be extreme.

Single Changes-over or paired studies have one simplifying peculiarity over the more ambitious Designs previously discussed in such detail and that is that it is commonly possible to complete them. Thus if one proposes 24 pairs of comparisons one can generally get them. This is because the requirements are so simple that even if some subject disappoints the experimenter by not completing his comparison it is very often possible to get a replacement even if a little later. Accordingly, this is one field where we shall have little to concern ourselves with incomplete design.

The paired test may be regarded as the extreme of Youdenish designs. This paired comparison will, in general, of course, require more than t lines to compare t Treatments. It is often, however, such a convenient Design that it is the only situation where one should not be unduly concerned about the number of pairs that may be necessary. It does seem desirable to make all comparisons as nearly as possible the same number of times. For this it is necessary to have C_2^t pairs, as previously observed. There is a considerable literature on paired comparisons when more than 2 Treatments are involved. It is usually referred to as Desig with 2 units per block. Thus Cox (1958) gives an elaborate table of suc Designs. His discussion is not restricted to the balanced cases which seem here preferable. Nor is he concerned with Change-over. His material and methods are discussed to some extent in the concluding section of this Chapter, under the heading of missing data situations.

Design with single Change-over - Paired studies or those with single Change-over, i.e., each subject may try 2 Treatments, are the only situation where it seems desirable to go into multiple designs, i.e., as in Equ. (171), where $r = gt$, $g \geq 2$, beyond a double Youden. The number of Rows is g times the number of Treatments. There are a few paired studies indicated in Table IV but since the value of g , there, is arbitrarily restricted to 3, there are very few cases. Here we are concerned with any arrangement necessary to test properly t Treatments and g will have to be as big as is necessary. On the other hand it is unnecessary to search diligently, as in the cases of $t > 2$, for solutions, we can always write out a Design on general principles and need only consider such Designs.

The number of Rows for such cases of t even, as occur in Table IV, and quite generally, is

$$r = 2C_2^t = t(t - 1) \tag{204}$$

whence

$$g = (t-1) \tag{205}$$

with fill of $f = 2$. Appropriate design may be illustrated most briefly by the examples,

4x2x12	g=3
Row I	(1) (2)
Δ	1
V	(1) (3)
Δ	2
IX	(1) (4)
Δ	3

6x2x30	g=5
Row I	(1) (2)
Δ	1
VII	(1) (3)
Δ	2
XIII	(1) (4)
Δ	3
XIX	(1) (5)
Δ	4
XXV	(1) (6)
Δ	5

In more detail, together with data, we have for the former case

	Period	
Row	1	2
I	(1) y_{111}	(2) y_{122}
II	(2) y_{212}	(3) y_{223}
III	(3) y_{313}	(4) y_{324}
IV	(4) y_{414}	(1) y_{421}
V	(1) y_{511}	(3) y_{523}
VI	(2) y_{612}	(4) y_{624}

	Period	
Row	1	2
VII	(3) y_{713}	(1) y_{721}
VIII	(4) y_{814}	(2) y_{822}
IX	(1) y_{911}	(4) y_{924}
X	(2) $y_{10,12}$	(1) $y_{10,21}$
XI	(3) $y_{11,13}$	(2) $y_{11,22}$
XII	(4) $y_{12,14}$	(3) $y_{12,23}$

It may be noted again that this Design can be rearranged into the form,

Row	Period 1	Period 2
I	(1)	(2)
VII	(3)	(1)
IX	(1)	(4)
II	(2)	(3)
VIII	(4)	(2)
III	(3)	(4)

Row	Period 1	Period 2
X	(2)	(1)
V	(1)	(3)
IV	(4)	(1)
XI	(3)	(2)
VI	(2)	(4)
XII	(4)	(3)

when plainly all comparisons occur twice and twice only $(f = 2)$ and that each Treatment occurs an equal number of times in each Period. It will be further noted that in this Design the Change-over is balanced, with every Treatment following (or preceded by) every other Treatment. For serious experimentation, however, it is best to use the form of writing in cyclic Columns and probably with the abridgement previously indicated. Such Designs will, generally, be of the character:

Row I	(1)	(2)
Δ		1
Row t+1	(1)	(3)
Δ		2
Row 2t+1	(1)	(4)
Δ		3
. . . . . .	. . . . .	
Row $(t-1)^2$	(1)	(t)
Δ		t-1

The cases of 4x2x12 and 6x2x30, previously shown, are examples of this. In practice some multiple of $2C_2^t$ may, of course, be employed.

The number of Rows for such cases of t odd, as occur in Table IV, and quite generally, is

$$r = C_2^t = \frac{t(t - 1)}{2} \tag{206}$$

whence

$$g = (t-1)/2 \tag{207}$$

with fill of $f = 1$. Appropriate design may be illustrated by the example of $t = 5$,

i.e., 5x2x10, briefly as follows:

Row I	(1)	(2)
Δ		1
VI	(1)	(4)
Δ		3

In more detail, together with data, we have

	Period	
Row	I	2
I	(1) y_{111}	(2) y_{122}
II	(2) y_{1222}	(3) y_{223}
III	(3) y_{133}	(4) y_{324}
IV	(4) y_{144}	(5) y_{425}
V	(5) y_{155}	(1) y_{521}

	Period	
Row	1	2
VI	(1) y_{161}	(4) y_{624}
VII	(2) y_{172}	(5) y_{725}
VIII	(3) y_{183}	(1) y_{821}
IX	(4) y_{194}	(2) y_{922}
X	(5) $y_{1,10,3}$	(3) $y_{10,2,3}$

This Design may again be rearranged into the form

	Period	
Row	1	2
I	(1)	(2)
VIII	(3)	(1)
VI	(1)	(4)
V	(5)	(1)
II	(2)	(3)

	Period	
Row	1	2
IX	(4)	(2)
VII	(2)	(5)
III	(3)	(4)
X	(5)	(3)
IV	(4)	(5)

where plainly all comparisons occur once and once only $(f = 1)$ and that a given Treatment occurs equally in each Column. Such must obviously be the case for all t even in a Design of this kind. Further, it will be noted that Change-over is unrepeated but not balanced. This is to say that no Treatment ever follows any other Treatment twice but it does not follow all other Treatments. For balance of Change-over one would write a more extensive Design, to wit,

Row I	(1)	(2)
Δ		1
IV	(1)	(3)
Δ		2
XI	(1)	(4)
Δ		3
XVI	(1)	(5)
Δ		4

This is, of course, a Design with $2C_2^t$ Rows just as for the case of t even. Designs of type with C_2^t Rows will generally be of the character:

Row	I	(1)	(2)
	Δ		1
Row	t+1	(1)	(4)
	Δ		3
. . . .	. . . .	. . .	. . .
Row	$\frac{(t-1)(t-2)}{2}$	(1)	(t-1)
	Δ		t-1

The case of 5x2x10, previously shown, is an example of this. Designs of type with $2C_2^t$ Rows will have the same character as those for t , even, previously discussed. Either type may in practice appear as some multiple of C_2^t or $2C_2^t$, respectively.

All these Designs have unrepeated Change-over, i.e., no Treatment is preceded by any other Treatment more than once and they all have Treatments in cyclic order in the Columns.

Note that a 2(3x2x3) was illustrated and analyzed in Table XXXII.

The very special case of $t = 2$ in a paired or single change-over Design is reserved to a later special section.

Analysis when there is no conditioning Period in the Design - Consider the most typical and basic kind of paired study, when there is no conditioning Period and there is no repetition of the comparisons. An example is provided in Table XXXVII, of 6x2x30. The number of Rows, r, is as in Equ. (204). In terms of the more general discussion earlier, $g = 5$, i.e., there are 5 times as many Rows as there are Treatments.

We conceive that for the paired comparison there exists the model first introduced in connection with the latin square. For Column 2 (Columns after the 1st) there may be a model, as in Equ. (43), involving both direct treatment effects and Carries-over. On the other hand in the 1st Column there is only allowed, as in Equ. (7) the direct effect. It is a matter, again, of supposing that any effect of background Carry-over is common to all Column 1 and hence completely confounded with it.

The data of the present experiment may be handled the same as those of any other Youdenish Design. There may be set up a matrix of equations as previously or the program of the Appendix may be used. Setting up least squares equations for the effect of Rows (pairs), one gets

$$\left.\begin{aligned} 2\hat{\alpha}_1 + \hat{\gamma}_1 + \hat{\gamma}_2 &= y_{11} + y_{12} = 5.7 \\ 2\hat{\alpha}_2 + \hat{\gamma}_2 + \hat{\gamma}_3 &= y_{21} + y_{22} = 6.3 \\ &\ldots\ldots\ldots\ldots\ldots \\ 2\hat{\alpha}_{18} + \hat{\gamma}_5 + \hat{\gamma}_6 &= y_{18,1} + y_{18,2} = 6.2 \end{aligned}\right\} \qquad (208)$$

when the subscripts on the observations y are understood to indicate the Row and Column of their occurrence. Also setting up the least squares equation for Treatment (1), one gets

$$\begin{aligned} &10\hat{\gamma}_1 + \hat{\alpha}_1 + \hat{\alpha}_6 + \hat{\alpha}_7 + \hat{\alpha}_{11} + \hat{\alpha}_{13} + \hat{\alpha}_{16} + \hat{\alpha}_{19} + \hat{\alpha}_{21} + \hat{\alpha}_{25} + \hat{\alpha}_{26} \\ &\quad = y_{11} + y_{62} + y_{71} + y_{11,2} + y_{13,1} + y_{16,2} + y_{19,1} + y_{21,2} + y_{25,1} + y_{26,2} \end{aligned} \qquad (209)$$

and similarly for each other Treatment. The overall effect $\hat{\mu}$ is simply the corner average 3.212. The effect of Period is easily found. Thus that for Period 2 is

$$\begin{aligned} \hat{\beta}_2 &= \frac{1}{r}\Sigma y_{i2k\ell} - \hat{\mu} \\ &= 3.187 - 3.212 \\ &= -.125 \end{aligned} \qquad (210)$$

because Columns are precisely balanced or orthogonal to all other effects. Such a matrix would, however, be obviously insupportable. One must rely on the results from the computer program in the Appendix or use explicit methods of solution, as discussed below.

In the case of paired comparisons, when only 2 Treatments occur in a Row, it is possible, however, to take steps that are more difficult in the larger Designs. One might use the special process if one were doing the analysis on a desk calculator. Thus consider the observation y_{i1k} in the $i^{th}(i = 1 \ldots 2C_2^t)$ Row, the 1^{st} Column, under the $k^{th}(k = 1 \ldots t, t \geq 4$ and even) Treatment and also the observation $y_{i2k\ell}$ under the ℓ^{th} $(l = 1 \ldots t)$ Carry-over, from an experiment. The business may be handled by considering the row differences

$$\left.\begin{array}{l} d_i = y_{i2k\ell} - y_{i1k'} \\ \qquad\qquad (k' \neq k \\ \qquad\qquad\ \ \ell = k') \\ d_i' = y_{i2k'\ell} - y_{i1k} \\ \qquad\qquad (\ell = k) \end{array}\right\} \tag{211}$$

of result for a given Treatment (k), in Period 2 less that in Period 1. Thus for Treatment (1) in Table XXXVIIa, the successive values of d_i, where (1) is in the second Period, are -.1, .0, -.2 etc. and of d_i' are -.3, -.3, -.3, -.1 etc. The primes indicate that Treatment (1) occurred in the first Period. It may be assured that these differences, such as

$$\left.\begin{array}{l} d_1' = y_{1221} - y_{111} = \gamma_2 - \gamma_1 + \delta_1 + 2\beta_2 + \varepsilon_1 \\ d_2 = y_{2213} - y_{213} = \gamma_1 - \gamma_3 + \delta_3 + 2\beta_2 + \varepsilon_2 \end{array}\right\} \tag{212}$$

with respect to Treatment (1), can be made readily enough to yield least squares estimates of the parameters γ_1 and δ_1. In general

$$\left.\begin{aligned}\hat{\gamma}_k &= \{(t - 1)\Sigma d_i + \Sigma d_i' - 2t(t - 1)\hat{\beta}_2\}/t(t - 2) \\ \hat{\delta}_k &= \{\Sigma d_i + \Sigma d_i' - 4(t - 1)\hat{\beta}_2\}/(t - 2)\end{aligned}\right\} \qquad (213)$$

Now we may get $\hat{\gamma}_k$ and $\hat{\delta}_\ell (\ell = k)$ for all k by noting the differences for the Rows in which it occurs and whether or not it occurs in the first Column. In the case where the complete Design of $2C_2^t$ is repeated g times these results generalize to

$$\left.\begin{aligned}\hat{\gamma}_k &= \{(t - 1)\Sigma d_i + \Sigma d_i' - 2gt(t - 1)\hat{\beta}_2\}/2gt(t - 2) \\ \hat{\delta}_\ell &= \{\Sigma d_i - \Sigma d_i' - 4g(t - 1)\hat{\beta}_2)\}/g(t - 2)\end{aligned}\right\} \qquad (214)$$

$$(\ell = k)$$

For the model of Equ. (7), without consideration of possible Carry-over, the least squares estimate for Treatment may be illustrated by

$$\hat{\gamma}_k \text{ or } (\hat{k}) = (\Sigma d - \Sigma d')/2t \quad . \qquad (215)$$

In the case when the complete Design of $2C_2^t$ is repeated g times

$$\hat{\gamma}_k = (\Sigma d - \Sigma d')/2gt \quad . \qquad (216)$$

In illustration consider the estimates from the data of Table XXXVII for Treatment alone. From Equ. (215)

$$(\hat{1}) \text{ or } \hat{\gamma}_1 = \{- .1 + .0 - .2 - .1 + .1 - (- .3 - .3 - .3 - .1 + .1)\}/12$$

$$= + .050 \quad . \qquad (217)$$

For all k , the results are as shown in Table XXXVIIb. The result is an application, essentially, of Equ.(112), that was used for Youden rectangles. From that,

$$
\begin{aligned}
(\hat{1}) \text{ or } \hat{\gamma}_1 &= (2\Sigma y_{ij1} - T_i)/12 \\
&= \{2(32.7) - 64.8\}/12 \\
&= .050 \quad .
\end{aligned}
\qquad (218)
$$

Further, from Equ. (210),

$$
\hat{\beta}_1 = 97.1/30 - 192.7/60 = + .025 \quad . \qquad (219)
$$

Again calculating from Equ. (213) for direct treatment effects, when allowance is made for possible Carry-over,

$$
\begin{aligned}
(\hat{1}) \text{ or } \hat{\gamma}_1 = \{&- .3 - .3 - .3 - .1 + .1 + 5(- .1 + .0 - .2 - .1 + .1) \\
&- 2(6)(5)(- .025)\}/6(4) \\
= &- .038
\end{aligned}
\qquad (220)
$$

as shown in Table XXXVIIIc together with the corresponding results for other Treatments and with the adjusted mean, as previously. For this model, the estimate of Carry-over of Treatment (1) is from Equ. (213),

$$
\begin{aligned}
[\hat{1}] \text{ or } \hat{\delta}_1 = \{&- .3 - .3 - .3 - .1 + .1 - .1 + .0 - .2 - .1 + .1 \\
&- 4(5)(- .025)\}/4 \\
= &- .175
\end{aligned}
\qquad (221)
$$

as shown in Table XXXVIIIc together with the corresponding results for other Carries-over.

In paired studies, it is often very easy to set up the paired units so that the Design is generally complete and repeated. In fact, we often do. In general, we may have g repetitions to give us say gt (t-1) Rows for t even. Thus, in a study of 6 Treatments we might have 5(6x2x30), or 5 replicates. Such procedure should greatly improve the quality of Treatment estimates.

Conceiving Table XXXVII as essentially one of 30 differences, each written in terms of β_2, γ_k, (perhaps δ_ℓ) and $\varepsilon_{ijk\ell}$, there may be written out a test of significance for effects in terms of pair differences, but let us content ourselves with the business of estimating Treatment and carry-over effects. Such elaboration along lines differing from the general situation for Youden rectangles might confuse our general reader and thus do more harm than good.

The handling of paired or single change-over experiments by the consideration of the difference within the pairs, as above, does make it possible and indeed fairly easy to analyze for direct Treatment and carry-over effects at least in situations such as the above. It must be noted that the perfect execution of the Design is important; otherwise, the solution of the equations becomes intolerable. Such advantage is actually trivial because mainly anyone interested in such a problem would use, on an electronic computer, the program in the Appendix.

For the paired Design, i.e., with single Change-over per experimental unit, when t is odd but there is a total of $2C_2^t$ Rows with each Treatment followed (or preceded) by all other Treatments, everything is much as just discussed for the case of t even. The differences may be conventionalized as in Equ. (221); the estimates of effect of Treatment alone are as in Equ. (215); the estimates for the more elaborate model are as in Equ. (213).

For the paired Design, i.e., with single Change-over per experimental unit, when t is odd but there is a total of only C_2^t Rows, as discussed in the immediately preceding section explicit solution is much more involved than for the well-regulated case of $2C_2^t$ Rows. A given Treatment can only be followed (or preceded) by half the other Treatments. Carry-over is unrepeated but not balanced. Of course, for the model, without concern for Carry-over, the estimates of effect of Treatment are simple, being

$$\hat{\gamma}_k \text{ or } (\hat{k}) = (\Sigma d - \Sigma d')/t \qquad (222)$$

like Equ. (215). For the more elaborate model, including Carry-over, the explicit statement for any given value of t is very heavy and for t in general onerous.

Such a paired study may be analyzed as a Youden. As previously, there could be set up a matrix for the equations relating $\hat{\mu}$, all $\hat{\alpha}_i$, $\hat{\beta}_1$, all $\hat{\gamma}_k$ and all $\hat{\delta}_\ell$. It would be very extensive and tedious. The really important relations are shown in Table XXXVIId where it is plain that direct treatment and carry-over effects are in large measure independent. Of course, half the observations, from the first Column of the Design, have no carry-over effect. Such a matrix would also teach us that a very large part of the experimental evidence was going to the estimation of Row effects, a control factor, and without experimental value. In practice, one would, probably, use the general Youden analysis, as in the Appendix, for an electronic computer. This program is organized so that the multiplicity of Rows does not essentially affect the size of the operation. The results of such operation are shown in Tables XXXVIIb and c.

Table XXXVII - Satisfaction with 6 Treatments over 2 Periods on 30 Groups

a. Data

Group	Period 1	Period 2	Sum	Diff.
I	(1) 3.0	(2) 2.7	5.7	-.3
II	(2) 3.0	(3) 3.3	6.3	+.3
III	(3) 2.6	(4) 3.0	5.6	+.4
IV	(4) 3.3	(5) 3.0	6.3	-.3
V	(5) 3.1	(6) 2.7	5.8	-.4
VI	(6) 3.0	(1) 2.9	5.9	-.1
VII	(1) 3.1	(3) 2.8	5.9	-.3
VIII	(2) 3.2	(4) 2.7	5.9	-.5
IX	(3) 2.1	(5) 2.8	4.9	+.7
X	(4) 2.9	(6) 3.4	6.3	+.5
XI	(5) 3.5	(1) 3.5	7.0	.0
XII	(6) 3.7	(2) 3.8	7.5	+.1
XIII	(1) 3.5	(4) 3.2	6.7	-.3
XIV	(2) 3.5	(5) 3.5	7.0	.0
XV	(3) 3.8	(6) 3.0	6.8	-.8
XVI	(4) 3.1	(1) 2.9	6.0	-.2
XVII	(5) 3.3	(2) 3.2	6.5	-.1
XVIII	(6) 3.4	(3) 3.6	7.0	+.2

Group	Period 1	Period 2	Sum	Diff.
XIX	(1) 3.3	(5) 3.2	6.5	-.1
XX	(2) 3.3	(6) 3.4	6.7	+.1
XXI	(3) 3.5	(1) 3.4	6.9	-.1
XXII	(4) 3.1	(2) 3.6	6.7	+.5
XXIII	(5) 3.3	(3) 3.2	6.5	-.1
XXIV	(6) 3.3	(4) 3.5	6.8	+.2
XXV	(1) 3.8	(6) 3.9	7.7	+.1
XXVI	(2) 3.2	(1) 3.3	6.5	+.1
XXVII	(3) 2.9	(2) 3.1	6.0	+.2
XXVIII	(4) 3.7	(3) 3.1	6.8	-.6
XXIX	(5) 3.2	(4) 3.1	6.3	-.1
XXX	(6) 3.4	(5) 2.8	6.2	-.6
Sum	97.1	95.6	192.7	-1.5
Mn.	3.237	3.187	3.212	

b. Analysis for Treatments without allowance for Carry-over

	$(\hat{1})$	$(\hat{2})$	$(\hat{3})$	$(\hat{4})$	$(\hat{5})$	$(\hat{6})$
Contr.	+.050	+.033	-.075	-.017	+.033	-.025
Adj. Mn.	3.262	3.245	3.137	3.195	3.245	3.187

Factors	d.f.	Residual Variability (Squares)
$\hat{\mu}$, Rows & Columns (control)	29	1.78
Control factors plus Treatments	24	1.71

$$F_{5,24} = \frac{(1.78 - 1.71)/5}{1.71/24} = .20 \text{ N.S.}$$

c. Analysis for Treatment with allowance for Carry-over

	$(\hat{1})$	$(\hat{2})$	$(\hat{3})$	$(\hat{4})$	$(\hat{5})$	$(\hat{6})$
Contr.	-.038	+.146	-.025	-.004	-.029	-.050
Adj. Mn.	3.174	3.358	3.187	3.208	3.183	3.162

	$[\hat{1}]$	$[\hat{2}]$	$[\hat{3}]$	$[\hat{4}]$	$[\hat{5}]$	$[\hat{6}]$
Contr.	-.175	+.225	+.100	+.025	-.125	-.050

Factors	d.f.	Residual Variability (Squares)
$\hat{\mu}$, Rows, Columns & Carries-over (control)	24	1.66
Control factors plus Treatments	19	1.60

$$F_{5,19} = \frac{(1.66 - 1.60)/5}{1.60/19} = .15 \text{ N.S.}$$

d. Satisfaction by Treatment and by previous Treatment--A crude analysis

	Treatment								
After	(1)	(2)	(3)	(4)	(5)	(6)	Sum	Adj. Mn.	Contr.
(1)		2.7	2.8	3.2	3.2	3.9	15.8	3.17	-.02
(2)	3.3		3.3	2.7	3.5	3.4	16.2	3.25	+.06
(3)	3.4	3.1		3.0	2.8	3.0	15.3	3.03	-.16
(4)	2.9	3.6	3.1		3.0	3.4	16.0	3.19	.00
(5)	3.5	3.2	3.2	3.1		2.7	15.7	3.13	-.06
(6)	2.9	3.8	3.6	3.5	2.8		16.6	3.35	+.16
Bk.gr.	3.0	3.0	2.6	3.3	3.1	3.0			
	3.1	3.2	2.1	2.9	3.5	3.7			
	3.5	3.5	3.8	3.1	3.3	3.4			
	3.3	3.3	3.5	3.1	3.3	3.3			
	3.8	3.2	2.9	3.7	3.2	3.4	97.1		
Sum	32.7	32.6	30.9	31.6	31.7	33.2	192.7		
Adj.Mn.	3.27	3.27	3.07	3.16	3.16	3.34			
Contr.	+.06	+.06	-.14	-.05	-.05	+.13			

The F test for Treatments and Carries-over in Table XXXVII are rather surprisingly small. It is therefore, the more curious that they seem to show correspondence; the correlation coefficient is +.79.

With regard to the intermingling of carry-over effects and direct treatment effects in a 2-period experiment involving more than 2 Treatments, it may be observed that they can be separated, as is discussed elsewhere.

As was pointed out in earlier chapters on latin squares and Youden rectangles, it seems impractical to make estimates of treatment and carry-over effects explicitly in terms of observations. It is best to set up the least squares equations. The explicit possibilities are, however, interesting in that they warn us of what Designs are impossible. For instance, one cannot estimate both carry-over and direct treatment effects in 2x2x2 or 3x2x3 as has already appeared. Even, however, for the t x 2 x t (t great), the explicit solution in terms of observations y , a solution involving the full model is very heavy.

Consider next and last the situation for t even when there are $2C_2^t$ Rows. After 2x2x2, the next case is 4x2x4. The standard design for 4 Treatments, i.e.:

	Period	
Row	1	2
I	(1)	(2)
II	(3)	(1)
III	(1)	(4)
IV	(2)	(3)
V	(4)	(2)
VI	(3)	(4)
VII	(2)	(1)
VIII	(1)	(3)
IX	(4)	(1)
X	(3)	(2)
XI	(2)	(4)
XII	(4)	(3)

would work since there are but 19 parameters to be estimated from 24 observations. Plainly, treatment and carry-over effects can be estimated for all such paired comparison designs with $t \geq 4$, t even.

Consider first the situation for t odd where there are C_2^t Rows. After 3x2x3, the next case is 5x2x10. The standard design for 5 Treatments, i.e.:

	Period	
Row	1	2
I	(1)	(2)
II	(3)	(1)
III	(1)	(4)
IV	(5)	(1)
V	(2)	(3)
VI	(4)	(2)
VII	(2)	(5)
VIII	(3)	(4)
IX	(5)	(3)
X	(4)	(5)

would just work because there are 19 parameters to be estimated from 20 observations. Plainly, all effects, including Carry-over, could be estimated for all paired comparison designs with $t \geq 5$, t odd.

Analysis when there is a conditioning Period in the Design - A 2-Period or paired experiment may be prefaced by a conditioning Period, although as was earlier discussed in connection with Youden rectangles the difficulties of design are considerable. The difficulty is that it will not do to put the conditioning Period under the same Treatment as the first Period of the experiment proper. If one had done so in the case reported in Table XXXVII each Treatment would be preceded by itself 5 times but by each other Treatment but once. No doubt ingenuity could get around such problems in particular cases but would probably be better employed in making work some of the more simple procedures recommended in this book. Nonetheless cases may occasionally be produced and must be analyzed. Such a case, which appeared of itself, is reported in Table XXXVIII.

In Table XXXVIII, 5 Treatments were tried, with a conditioning Period. Treatments occur equally in each of the condition Period 0, and the proper experimental Periods 1 and 2. The Design shows, however, small departures from balance, characteristic of many experiments, as they are actually executed. For instance, the comparisons (Columns 1 and 2) are:

vs.	(1)	(2)	(3)	(4)	(5)
(1)		3	5	5	3
(2)	3		3	5	5
(3)	5	3		3	5
(4)	5	5	3		3
(5)	3	5	5	3	

The Carries-over are as balanced as is possible. They are:

	Treatment				
After	(1)	(2)	(3)	(4)	(5)
(1)	4	3	3	3	3
(2)	3	4	3	3	3
(3)	3	3	4	3	3
(4)	3	3	3	4	3
(5)	3	3	3	3	4

Let us consider first the model with only direct treatment effects. For this it is perhaps necessary to form the estimates

$$\hat{\mu} = 5421/80$$
$$= 67.76 \tag{223}$$

and

$$\hat{\beta}_1 = 2733/40 - \hat{\mu}$$
$$= + .56 \tag{224}$$

The estimate of the effect of Treatment (1) is gotten from the consideration of all differences involving (1) with a correct sign. The result is:

$$20\hat{\gamma}_1 + \hat{\gamma}_2 - \hat{\gamma}_3 - \hat{\gamma}_4 + \hat{\gamma}_5 = -17 \tag{225}$$

and similar least squares equations can be set up for $(\hat{2})$, $(\hat{3})$, $(\hat{4})$ and $(\hat{5})$. It would be logically possible to write out for the situation of Table XXXVIII a matrix of all the least squares equations but it would be very massive, because there are 40 Rows which are very empty because there are so few data in those Rows. The program for electronic computer, shown in the Appendix may, however, be used because it has been particularly designed to be free of the question of the number of Rows.

Using the program of the Appendix, based upon the usual least squares equations, the estimates and the test of significance are shown in Table XXXVIIIb. The addition of Carry-over to the model much increases the value of F assignable to Treatments although it still just misses significance.

Table XXXVIII - A 2-Period experiment

a. Design and data

	Period			
Group	0	1	2	Sum
I	((1))	(1) 68	(2) 63	131
II	((2))	(2) 66	(3) 64	130
III	((3))	(3) 70	(4) 70	140
IV	((4))	(4) 64	(5) 56	120
V	((5))	(5) 75	(1) 70	145
VI	((2))	(1) 60	(3) 65	125
VII	((3))	(2) 74	(4) 71	145
VIII	((4))	(3) 78	(5) 81	159
IX	((5))	(4) 80	(1) 76	156
X	((1))	(5) 69	(2) 76	145
XI	((2))	(1) 62	(4) 70	132
XII	((4))	(2) 66	(5) 65	131
XIII	((4))	(3) 65	(1) 62	127
XIV	((5))	(4) 53	(2) 56	109
XV	((1))	(5) 64	(3) 67	131
XVI	((3))	(1) 73	(4) 65	138
XVII	((3))	(3) 73	(5) 73	146
XVIII	((3))	(2) 66	(5) 58	124
XIX	((5))	(3) 63	(1) 63	126
XX	((1))	(1) 71	(2) 69	140

	Period			
Group	0	1	2	Sum
XXI	((2))	(2) 63	(3) 64	127
XXII	((3))	(3) 64	(4) 70	134
XXIII	((4))	(4) 66	(1) 53	119
XXIV	((1))	(4) 56	(2) 61	117
XXV	((2))	(1) 73	(3) 68	141
XXVI	((2))	(2) 68	(3) 69	137
XXVII	((5))	(5) 76	(2) 71	147
XXVIII	((2))	(5) 63	(3) 65	128
XXIX	((3))	(2) 67	(4) 68	135
XXX	((4))	(3) 73	(5) 58	131
XXXI	((1))	(5) 76	(2) 67	143
XXXII	((1))	(1) 65	(2) 66	131
XXXIII	((2))	(2) 66	(4) 68	134
XXXIV	((4))	(4) 69	(5) 69	138
XXXV	((5))	(5) 83	(1) 81	164
XXXVI	((1))	(1) 68	(3) 70	138
XXXVII	((3))	(3) 68	(4) 68	136
XXXVIII	((4))	(4) 69	(5) 66	135
XXXIX	((5))	(5) 61	(1) 68	129
XL	((5))	(4) 79	(1) 78	157
Sum		2733	2688	5421

b. Analysis for Treatments without allowance for Carry-over

	$\widehat{(1)}$	$\widehat{(2)}$	$\widehat{(3)}$	$\widehat{(4)}$	$\widehat{(5)}$
Contr.	-.69	+.20	+.61	+1.37	-1.49
Adj. Mn.	67.07	67.96	68.37	69.13	66.27

Factors	d.f.	Residual Variability (Squares)
$\hat{\mu}$, Rows & Columns (control)	39	532.13
Control factors plus Treatments	35	480.15

$$F_{4,35} = \frac{(532.13 - 480.15)/4}{480.15/35} = .95 \text{ N.S.}$$

c. Analysis for Treatments with allowance for Carry-over

	$\hat{(1)}$	$\hat{(2)}$	$\hat{(3)}$	$\hat{(4)}$	$\hat{(5)}$
Contr.	-1.92	-1.96	+.58	+3.60	-.31
Adj. Mn.	65.84	65.80	68.34	71.36	67.45

	$\hat{[1]}$	$\hat{[2]}$	$\hat{[3]}$	$\hat{[4]}$	$\hat{[5]}$
Contr.	-4.24	-2.82	+3.55	+4.79	-1.28

Factors	d.f.	Residual Variability (Squares)
$\hat{\mu}$, Rows, Columns & Carries-over (control)	35	477.78
Control factors plus Treatments	31	371.81

$$F_{4,31} = \frac{(477.78 - 371.81)/4}{371.81/31} = 2.21 \text{ N.S.}$$

Factors	d.f.	Residual Variability (Squares)
$\hat{\mu}$, Rows, Columns & Treatments (control)	35	480.15
Control factors plus Carries-over	31	371.81

$$F_{4,31} = \frac{(480.15 - 371.81)/4}{371.81/31} = 2.26 \text{ N.S.}$$

d. Results by Treatment and by previous Treatment--A crude analysis

	Treatment							
After	(1)	(2)	(3)	(4)	(5)	Sum	Adj. Mn.	Contr.
(1)	68 71 65 68 =272	63 69 66 =198	65 68 70 =203	56 70 65 =191	69 64 76 =209	1073	67.03	-.73
(2)	60 62 73 =195	66 63 68 66 =263	64 64 69 =197	71 68 68 =207	63 65 58 =186	1048	65.57	-2.19
(3)	73 62 63 =198	74 66 67 =207	70 73 64 68 =275	70 70 68 =208	81 73 58 =212	1100	68.75	+.99
(4)	76 53 78 =207	66 56 61 =183	78 65 73 =216	64 66 69 69 =268	56 69 66 =191	1065	66.55	-1.21
(5)	70 81 68 =219	76 71 67 =214	63 67 65 =195	80 53 79 =212	75 76 83 61 =295	1135	70.92	+3.16
Sum	1091	1065	1086	1086	1093	5421		
Adj. Mn.	68.23	66.70	67.81	67.95	68.12		67.76	
Contr.	+.47	-1.06	+.05	+.19	+.36			

It may be noted that the correspondence between Carries-over and Treatments was again high in Table XXXVIII. The correlation between the two kinds of estimate was +.92.

Procedure when there are 2 Treatments - One of the commonest problems in experimentation, that of deciding between 2 Treatments, is, unfortunately, particularly intractable. It is practically impossible to test them in 2 Periods. The design most commonly attempted is discussed immediately below. It is employed, to great mischief, for it tends to give erroneous results. This special case has such peculiar difficulties and yet is of such great practical importance that it is discussed below in a particularly thorough way. It becomes necessary to get away from Designs in 2 Columns but the matter is best cleared up here. It is possible and often both practical and convenient to write special 2-treatment Designs that make it possible to estimate direct treatment effect free of Carry-over. Two such are shown immediately below. They illustrate, incidentally, the importance of Carry-over.

The situation for the smallest of latin squares ,2x2x2, is of particular interest because it is impossible of analysis, is much beloved by "practical" experimenters and must lead to many false decisions. For this 2-treatment Design, it is impossible to estimate Carry-over, if it occurs to free the estimate of direct treatment effect of it. The point may be seen algebraically. The typical, "straightforward" Design, commonly used for 2 Treatments, is

	Period	
Subject	1	2
I	(1) y_{111}	(2) y_{1221}
II	(2) y_{212}	(1) y_{2212}
III	(1) y_{311}	(2) y_{3221}
IV	(2) y_{412}	(1) y_{4212}
V	(1) y_{511}	(2) y_{5221}
VI	(2) y_{612}	(1) y_{6212}
	etc.	

which may give results much influenced by Carry-over, since Carry-over [1] is always confounded with direct Treatment effect (1) and [2] with (2). Regardless of the number of repetitions, there are really only 4 kinds of thing, (1) or (2) each preceded by some general kind of background and (1) and (2) each preceded by the other. It may be conceived that there obtains the model of Equ. (43) with Carry-over. Then, it is immediately apparent, bearing in mind Equ. (9) and (44) that it is required to estimate from the above data, with their 4 observations, y , 5 parameters, μ , α_1 , β_1 , γ_1 and δ_1 , but this is impossible. If the least squares estimates are attempted, it will be found that there exists no solution for δ_1 . There are too many parameters in the model for them to be estimated by 4 things. This means, of course, that there is no solution by any method, explicit or otherwise. If one boldly ignores the possible Carry-over and estimates direct treatment effects, as is usually done by "practical" men,one gets for, say, Treatment (1)

$$(y_{1110} + y_{2212} - y_{1221} - y_{2120})/4 = \hat{\gamma}_1 - \frac{\hat{\delta}_1}{2} \tag{226}$$

so that it is still a mixture of direct Treatment effect and Carry-over. Yet this is precisely the estimate gotten in a great deal of experimental work. If, as has been illustrated and is often the case, [1] and (1) are strongly positively correlated such a Design will tend to make (1) judged bad if it is relatively good and (2) judged good if it is relatively bad. Such a test is undesirable. The situation is the worse because it is impossible to distinguish algebraically or arithmetically between direct treatment effect and Carry-over. Nor here, does it help a whit to repeat the Design, so far as this problem of underdetermination exists. The repetition may be very valuable, of course, in estimating extraneous variability.

For illustration the data of Table V, used previously to illustrate the nature and existence of Carry-over, may be used in part. For this purpose, consider those of the original Week 2 and Week 4. This is an entirely legitimate type of experiment and often a very good one. It is as if a given Treatment were applied for two weeks and an observation made in the second Week. In this way any Carry-over in the first Week of the fortnight is dropped. Also if one is lucky and gets positive Carry-over (as actually obtained in this case) the effect of Treatment is heightened by the superimposition of effect of Carry-over on that of unit Treatment. As can be seen in Table XXXIX, it is very clear. From Equ. (216), the estimate of effect of Treatment (1), without allowance for Carry-over is

$$\begin{aligned} \hat{\gamma}_1 \text{ or } (\hat{1}) &= (-12.6 - 87.1)/24 \\ &= -4.154{,}1\dot{6} \ . \end{aligned} \qquad (227)$$

The estimate of Treatment (2) is then + $4.154{,}1\dot{6}$, as shown in Table XXXIXb. If one attempts to estimate both Treatment and Carry-over from Equ. (213), one is stopped by the fact that $t - 2 = 0$. In the present problem it is, of course, impossible to go on to the analysis for effect of Treatment when allowance is made for Carry-over.

The actual analysis shown in Table XXXIXb was gotten by appeal to the general program for Youden Designs, as shown in the Appendix. It may, however, be profitable to consider the least squares equations underlying that, to wit:

$$\left.\begin{array}{cccccccccccccccccccr}
\hat{\mu} & \hat{\alpha}_1 & \hat{\alpha}_2 & \hat{\alpha}_3 & \hat{\alpha}_4 & \hat{\alpha}_5 & \hat{\alpha}_6 & \hat{\alpha}_7 & \hat{\alpha}_8 & \hat{\alpha}_9 & \hat{\alpha}_{10} & \hat{\alpha}_{11} & \hat{\alpha}_{12} & \hat{\beta}_1 & \hat{\gamma}_1 & \hat{\gamma}_2 & \hat{\delta}_1 & \hat{\delta}_2 & & \text{Sum} \\
\hline
24 & & & & & & & & & & & & & & & & & & = & 1533.7 \\
2 & 2 & & & & & & & & & & & & & & & 1 & & = & 126.9 \\
2 & & 2 & & & & & & & & & & & & & & & 1 & = & 152.5 \\
2 & & & 2 & & & & & & & & & & & & & 1 & & = & 115.9 \\
2 & & & & 2 & & & & & & & & & & & & & 1 & = & 145.8 \\
2 & & & & & 2 & & & & & & & & & & & 1 & & = & 96.0 \\
2 & & & & & & 2 & & & & & & & & & & & 1 & = & 120.0 \\
2 & & & & & & & 2 & & & & & & & & & 1 & & = & 138.2 \\
2 & & & & & & & & 2 & & & & & & & & & 1 & = & 132.6 \\
2 & & & & & & & & & 2 & & & & & & & 1 & & = & 132.4 \\
2 & & & & & & & & & & 2 & & & & & & & 1 & = & 125.6 \\
2 & & & & & & & & & & & 2 & & & & & 1 & & = & 126.1 \\
 & 1 & 1 & 1 & 1 & 1 & 1 & 1 & 1 & 1 & 1 & 1 & 1 & & & & & & = & .0 \\
12 & & & & & & & & & & & & & 12 & & & & & = & 729.6 \\
12 & & & & & & & & & & & & & & 12 & & & 6 & = & 717.0 \\
 & & & & & & & & & & & & & & 1 & 1 & & & = & .0 \\
6 & 1 & & 1 & & 1 & & 1 & & 1 & & 1 & & & & & 6 & & = & 411.3 \\
 & & & & & & & & & & & & & & & & 1 & 1 & = & .0
\end{array}\right\} \qquad (228)$$

(The positions left empty contain zeroes, not shown)

If now the lines due to consideration of δ_1 and δ_2 are suppressed the equations solve well enough to give the results of Table XXXIXb. If, however, the entire matrix is attempted a singularity is encountered which is but a restatement of the difficulty previously encountered in the consideration of the number of kinds of things and the number of parameters required or again in the inapplicability of Equ. (213).

Table XXXIX - Satisfaction by 2 kinds of Treatment over 2 Periods on 12 Groups

a. Data

	Week			
Group	2	4	Sum	Diff.
I	(1) 56.9	(2) 70.0	126.9	+13.1
II	(2) 75.0	(1) 77.5	152.5	+ 2.5
III	(1) 51.7	(2) 64.2	115.9	+12.5
IV	(2) 75.6	(1) 70.2	145.8	- 5.4
V	(1) 38.2	(2) 57.8	96.0	+19.6
VI	(2) 49.1	(1) 70.9	120.0	+21.8
VII	(1) 56.4	(2) 81.8	138.2	+25.4
VIII	(2) 70.6	(1) 62.0	132.6	- 8.6
IX	(1) 62.7	(2) 69.7	132.4	+ 7.0
X	(2) 67.8	(1) 57.8	125.6	-10.0
XI	(1) 58.3	(2) 67.8	126.1	+ 9.5
XII	(2) 67.3	(1) 54.4	121.7	-12.9
Sum	729.6	804.1	1533.7	+74.5

b. Analysis for Treatments, without allowance for Carry-over

	$(\hat{1})$	$(\hat{2})$
Contr.	-4.15	+4.15
Adj. Mn.	59.75	68.05

Factors	d.f.	Residual Variability (Squares)
$\hat{\mu}$, Rows & Columns (control)	11	942.46
Control factor plus Treatments	10	528.29

$$F_{1,10} = \frac{(942.46 - 528.29)/1}{528.29/10} = 7.84 \text{ N.S.}$$

c. Results by Treatment and by previous Treatment--a crude analysis

	Treatment				
After	(1)	(2)	Sum	Adj. Mn.	Contr.
(1)		70.0 81.8			
		64.2 69.7			
		57.8 67.8 = 411.3	411.3	61.79	-5.23
(2)	77.5 62.0				
	70.2 57.8				
	70.9 54.4 = 392.8		392.8	72.24	+5.22
Bk.gr.	56.9 56.4	75.0 70.6			
	51.7 62.7	75.6 67.8			
	38.2 58.3 = 324.2	49.1 67.3 = 405.4	729.6		
Sum	717.0	816.7	1533.7		
Adj.Mn.	57.14	70.67		63.90	
Contr.	-6.76	+6.77			

It might be objected that the data of Table XXXIX, from an analysis of variance point of view, could be handled by testing against the variability among Groups. This could, indeed, be done. It would be equivalent to dropping the elements of α_i in Equ. (228). It would further be precisely the thing done in Table XXXIXc. The number of degrees of freedom residual would be increased by 11 but generally such procedure would much inflate the mean residual variability.

If one must test 2 Treatments in 2 Periods, there could no doubt be found various Designs. One that suggests itself is

	Period	
Unit	1	2
I	(1)	(2)
II	(2)	(1)
III	(1)	(1)
IV	(2)	(2)

with perhaps repetition. It is objectionable because it is extravagant with experimental material. Units III and IV are devoted entirely to the determination of Carry-over which may not exist and if it does, may be of secondary interest. The best thing seems to be to employ Designs of more than 2 Periods which do work; two such are illustrated in the following discussion.

In the Design when a previous conditioning Period is judiciously employed the situation is much improved over the foregoing. Say the Design and data are:

		Period	
Group	0	1	2
I	((1))	(1) y_{1111}	(2) y_{1221}
II	((2))	(2) y_{2122}	(1) y_{2212}
III	((1))	(1) y_{3111}	(2) y_{3221}
IV	((2))	(2) y_{4122}	(1) y_{4212}
V	((1))	(1) y_{5111}	(2) y_{5221}
VI	((2))	(2) y_{6122}	(1) y_{6212}
		etc.	

where the model of Equ. (43), with allowance for Carry-over again obtains. Then $y_{1111} + y_{3111} + y_{5111}$ plays the role of y_{1111} alone, etc., and one still has no estimate of direct treatment or carry-over effects. If one ignores the Carry-over--and what else can one do?--to simply estimate the effect of Treatment (1) by least squares estimate, one gets

$$\hat{\lambda}_1 = (y_{1111} + y_{2212} - y_{1221} - y_{2122})/4 \tag{229}$$

an estimate of the direct Treatment free of Carry-over. Still one cannot solve for $\hat{\delta}_1$ in terms of the full model embracing estimates of Rows etc., as previously employed. The problems of the insoluble 2x2x2 are nowise solved by repeating the Design to say a 3(2x2x2). In Table XL there is an example of actual results on the model just discussed. The data were gotten by simply cutting the real data from Weeks 1, 2 and 3 from Table V. The Groups have been renumbered and the data arranged to manifest the basic organization on 4 kinds of observations.

Table XL- Two Treatments tried by 12 Groups in 3 Weeks

a. Design and Results

Group	Week 0	 1	 2	Sum
I	((1))	(1) 56.9	(2) 59.4	116.3
II	((1))	(1) 51.7	(2) 61.7	113.4
III	((1))	(1) 38.2	(2) 52.7	90.9
IV	((1))	(1) 56.4	(2) 61.8	118.2
V	((1))	(1) 62.7	(2) 68.2	130.9
VI	((1))	(1) 58.3	(2) 60.7	119.0
VII	((2))	(2) 75.0	(1) 72.8	147.8
VIII	((2))	(2) 75.6	(1) 71.1	146.7
IX	((2))	(2) 49.1	(1) 61.8	110.9
X	((2))	(2) 70.6	(1) 60.0	130.6
XI	((2))	(2) 67.8	(1) 60.0	127.8
XII	((2))	(2) 67.3	(1) 67.3	134.6
Sum		729.6	757.5	1487.1
Mean		60.8	63.1	

b. Analysis for direct Treatment alone

	$(\hat{1})$	$(\hat{2})$
Contr.	-2.20	+2.20
Adj. Mn.	59.76	64.16

Factors	d.f.	Residual Variability (Squares)
$\hat{\mu}$, Rows & Columns (control)	11	338.13
Control factors plus Treatment	10	222.24

$$F_{1,10} = \frac{(338.13 - 222.24)/1}{222.24/10} = 5.21 \text{ N.S.}$$

c. Averages according to Treatment and previous Treatment--a crude analysis

	Treatment			
After	(1)	(2)	Mean	Contr.
(1)	54.0	60.8	57.4	-4.6
(2)	65.5	67.6	66.5	+4.5
Mean	59.8	64.2	62.0	
Contr.	-2.2	+2.2		

In Table XLb, a rough result was gotten by ignoring the possible effect of Rows and it was precisely this reduction in constants that made solution of a sort possible for Treatments and Carries-over. As a practical measure for the estimation of effects this device is not too unsatisfactory, provided that there are many Rows. The matter may even be formalized by dropping the contributions α_i in the model and then proceeding with least squares equations to a test of significance. Of course, the residual variability will consist in large part of that between Groups. This tends to be large and so the value of F would probably not be significant.

As a first example of a 2-treatment Design where the effect of direct Treatment and Carry-over can be separated reconsider the data of Table Va, which was used to introduce, in a loose and general way, the idea of Carry-over and which should now be analyzed more closely. This can be done by using the appropriate program for electronic computer as in the Appendix. Accordingly, Table XLI has been formed. It will be seen that the significance of Treatments has been somewhat heightened by the introduction of Carry-over into the model. The latter is significant in its own right.

Table XLI - Analysis of the data from Table V put in form used elsewhere

a. Design and results (repeated from Table V)

			Week			
Group	0	1	2	3	4	Sum
I	((2))	(1) 72.3	(1) 56.9	(2) 59.4	(2) 70.0	258.6
II	((1))	(2) 76.0	(2) 75.0	(1) 72.8	(1) 77.5	301.3
III	((2))	(1) 61.7	(1) 51.7	(2) 61.7	(2) 64.2	239.3
IV	((1))	(2) 75.6	(2) 75.6	(1) 71.1	(1) 70.2	292.5
V	((2))	(1) 36.4	(1) 38.2	(2) 52.7	(2) 57.8	185.1
VI	((1))	(2) 61.8	(2) 49.1	(1) 61.8	(1) 70.9	243.6
VII	((2))	(1) 65.5	(1) 56.4	(2) 61.8	(2) 81.8	265.5
VIII	((1))	(2) 57.8	(2) 70.6	(1) 60.0	(1) 62.0	250.4
IX	((2))	(1) 60.0	(1) 62.7	(2) 68.2	(2) 69.7	260.6
X	((1))	(2) 68.9	(2) 67.8	(1) 60.0	(1) 57.8	254.5
XI	((2))	(1) 63.8	(1) 58.3	(2) 60.7	(2) 67.8	250.6
XII	((1))	(2) 60.4	(2) 67.3	(1) 67.3	(1) 54.4	249.4
Sum		760.2	729.6	757.5	804.1	3051.4
Mean		63.4	60.8	63.1	67.0	63.6

b. Analysis for Treatments without allowance for Carry-over

	$\hat{(1)}$	$\hat{(2)}$
Contr.	-2.33	+2.33
Mean	61.24	65.90

factors	d.f.	Residual Variability (Squares)
$\hat{\mu}$, Groups, Weeks (control)	33	1419.0475
Control factors plus Treatments	32	1157.7142

$$F_{1,32} = \frac{1419.0475 - 1157.7142}{1157.7142/32} = 7.22^{*}$$

c. Analysis for Treatments with allowance for Carry-over

	$\widehat{(1)}$	$\widehat{(2)}$
Contr.	-2.33	+2.33
Mean	61.24	65.90

	$\widehat{[1]}$	$\widehat{[2]}$
Contr.	-1.82	+1.82

Factors	d.f.	Residual Variability (Squares)
$\hat{\mu}$, Groups, Weeks, Carries-over (control)	32	1259.9067
Control factors plus Treatments	31	998.5734

$$F_{1,31} = \frac{1259.9067 - 998.5734}{998.5734/31} = 8.11^{**}$$

Factors	d.f.	Residual Variability (Squares)
$\hat{\mu}$, Groups, Weeks, Treatments (control)	32	1157.7142
Control factors plus Carries-over	31	998.5734

$$F_{1,31} = \frac{1157.7142 - 998.5734}{998.5734/31} = 4.94^{*}$$

This Design is of curious interest because, since the effects there are entirely orthogonal, it is most convenient and quite legitimate to use the conventional analysis of variance,* along the lines indicated earlier in connection with the analysis of the latin square.

It may be of some interest to compare the clear-cut results of Table XLIc with those shown earlier in Tables XXXIX and XL, consisting of parts of the same data, and subject to crude analysis (without allowance for effects α_i). The earlier data suggested Carry-over agreeing with what seems to be the true direction of effect of Treatment.

As a second example of 2-Treatment experiment designed to deal with Carry-over consider the data of Table XLII. Not only is the order of Treatments a little different from that in Table Va but the conditioning week here is absent. For convenience of calculation, the results have been gathered so that each pattern with its five repetitions is together. This

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*For the model with Treatments but not Carry-over, we get the results:

Source	d.f.	Sum Square	Mean Square	F
Groups	11	2258.2342	205.2940	---
Weeks	3	236.8975	78.9658	---
Treatments	1	261.3333	261.3333	7.22*
Residual	32	1157.7142	36.1786	---
Total	47	3914.1792		

For the model with Carry-over in addition we get:

Source	d.f.	Sum Square	Mean Square	F
Groups	11	2258.2342	205.2940	---
Weeks	3	236.8975	78.9658	---
Treatments	1	261.3333	261.3333	8.11**
Carry-over	1	159.1408	159.1408	4.94*
Residual	31	998.5734	32.2120	
Total	47	3914.1792		

Design gives data more difficult of analysis than Table Va because direct treatment effect and Carry-over are in some degree confounded. Plainly (1) follows (2) more frequently than it follows itself. Accordingly direct treatment effect (1) is confounded in some measure with Carry-over [2] while (2) is in the same measure counfounded with [1]. The analysis may be handled by appeal to the program in the Appendix or essentially to the least squares Equ. (234) below. For the model with direct treatment effects alone, i.e., without Carry-over, since everything is orthogonal, the treatment estimates are simply averages and so are the row and column effects. Thus the estimate for μ comes from

$$80\hat{\mu} = 3112$$
$$\hat{\mu} = 38.90 \tag{230}$$

for Row I from

$$4\hat{\mu} + 4\hat{\alpha}_1 = 159$$
$$\hat{\alpha}_1 = +.85 \quad . \tag{231}$$

For Column 1 from

$$20\hat{\mu} + 20\hat{\beta}_1 = 776$$
$$\hat{\beta}_1 = -.10 \tag{232}$$

and for Treatment (1) from

$$40\hat{\mu} + 40\hat{\gamma}_1 = 168$$
$$\hat{\gamma}_1 = +1.3 \quad . \tag{233}$$

It, of course, follows that it may be convenient and quite legitimate to use the conventional analysis of variance, along the lines just indicated in connection with Table XLI or indicated earlier in connection with the analysis of the latin square.

The more involved but complete analysis consists of fitting by least squares the complete model involving estimates of both Treatments and Carries-over. The estimate $\hat{\mu}$ remains as in Equ. (230). The estimates for Column effects remain as previously, i.e., simple averages. For other effects, as in Table XLII, it is necessary to use the program in the Appendix or essentially to employ the least squares equations as follows:

$\hat{\mu}$	$\hat{\alpha}_1$	$\hat{\alpha}_2$	$\hat{\alpha}_3$	$\hat{\alpha}_4$	$\hat{\alpha}_5$	$\hat{\alpha}_6$	$\hat{\alpha}_7$	$\hat{\alpha}_8$	$\hat{\alpha}_9$	$\hat{\alpha}_{10}$	$\hat{\alpha}_{11}$	$\hat{\alpha}_{12}$	$\hat{\alpha}_{13}$	$\hat{\alpha}_{14}$	$\hat{\alpha}_{15}$	$\hat{\alpha}_{16}$	$\hat{\alpha}_{17}$	$\hat{\alpha}_{18}$	$\hat{\alpha}_{19}$	$\hat{\alpha}_{20}$	$\hat{\beta}_1$	$\hat{\gamma}_1$	$\hat{\gamma}_2$	$\hat{\delta}_1$	$\hat{\delta}_2$	Sum
80																										= 3112
4	4																							1		= 159
4		4																						1		= 142
4			4																					1		= 129
4				4																				1		= 175
4					4																			1		= 137
4						4																		1		= 148
4							4																	1		= 115
4								4																1		= 173
4									4															1		= 173
4										4														1		= 149
4											4														1	= 156
4												4													1	= 121
4													4												1	= 153
4														4											1	= 173
4															4										1	= 148
4																4									1	= 174
4																	4								1	= 184
4																		4							1	= 149
4																			4						1	= 172
	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1						= 0
20																					20					= 776
40																						40			10	= 1608
																						1	1			= 0
30	1	1	1	1	1	1	1	1	1	1											-10		10	30		= 1172
																								1	1	= 0

(239)

(The positions left empty contain zeroes, not shown.)

Table XLII - Two Treatments tried on 24 Groups

a. Data

Group	Week 1	Week 2	Week 3	Week 4	Sum
I	(1) 40	(1) 48	(2) 44	(2) 27	159
II	36	44	32	30	142
III	34	37	24	34	129
IV	43	40	48	44	175
V	34 = 187	39 = 208	42 = 190	22 = 157	137
VI	(1) 39	(2) 32	(1) 39	(2) 38	148
VII	28	34	26	27	115
VIII	42	44	47	40	173
IX	48	38	49	38	173
X	35 = 192	32 = 180	42 = 203	40 = 183	149
XI	(2) 34	(1) 39	(2) 38	(1) 45	156
XII	38	33	25	25	121
XIII	32	36	40	45	153
XIV	42	45	43	43	173
XV	42 = 188	33 = 186	38 = 184	35 = 193	148
XVI	(2) 39	(2) 42	(1) 47	(1) 46	174
XVII	51	48	37	48	184
XVIII	36	42	32	39	149
XIX	38	37	49	48	172
XX	45 = 209	44 = 213	47 = 212	46 = 227	182
Sum	776	787	789	760	3112
Mean	38.80	39.35	39.45	38.00	
Sum Sq.	30,694	31,471	32,349	30,132	

b. Analysis for Treatments without allowance for Carry-over

	$(\hat{1})$	$(\hat{2})$
Contr.	+1.30	-1.30
Adj. Mn.	40.20	37.60

Factors	d.f.	Residual Variability (Squares)
$\hat{\mu}$, Rows and Columns (control)	57	1637.50
Control factors plus Treatments	56	1502.30

$$F_{1,56} = \frac{1637.50 - 1502.30}{1502.30/56} = 5.04^*$$

c. Analysis for Treatments with allowance for Carry-over

	$(\hat{1})$	$(\hat{2})$
Contr.	+1.61	-1.61
Adj. Mn.	40.51	37.29

	$[\hat{1}]$	$[\hat{2}]$
Contr.	+1.24	-1.24

Factors	d.f.	Residual Variability (Squares)
$\hat{\mu}$, Rows, Columns & Carry-over (control)	56	1613.76
Control factors plus Treatment	55	1425.20

$$F_{1,55} = \frac{(1613.76 - 1425.20)/1}{1425.20/55} = 7.28^{**}$$

Factors	d.f.	Residual Variability (Squares)
$\hat{\mu}$, Rows, Columns,& Treatments (control)	56	1502.10
Control factors plus Carry-over	55	1425.20

$$F_{1,55} = \frac{(1502.10 - 1425.20)/1}{1425.20/55} = 2.97 \text{ N.S.}$$

d. Averages according to Treatment and previous Treatment--a crude analysis

	Treatment				
After	(1)	(2)	Sum	Adj. Mn.	Contr.
(1)	208	190			
	227	180			
		183			
		184	1172	39.2	+.2
(2)	203	157			
	186	213			
	193				
	212		1164	38.7	-.3
Bk.gr.	187	188			
	192	209	776		
Sum	1608	1504	3112		
Adj.Mn.	40.2	37.6		38.9	
Contr.	+1.3	-1.3			

Analysis when there are 3 Treatments - The smallest Youden rectangle, $c < t$, is the Yates Design 3x2x3, i.e.:

	Period	
Row	1	2
I	(1) y_{111}	(2) y_{1221}
II	(2) y_{212}	(3) y_{2232}
III	(3) y_{313}	(1) y_{3213}

would not work because it involves 8 parameters:

$\hat{\mu}$	1	Treatments	2
Rows	2	Carries-over	2
Columns	1		

and has but 6 observations. It might be replaced by the type of design used for paired comparisons with an even value of t, i.e.:

	Period	
Row	1	2
I	(1) y_{111}	(2) y_{1221}
II	(2) y_{212}	(3) y_{2232}
III	(3) y_{313}	(1) y_{3213}
IV	(1) y_{411}	(3) y_{4231}
V	(2) y_{512}	(1) y_{5212}
VI	(3) y_{613}	(2) y_{6223}

where there are 11 parameters that can be estimated from the 12 observations. This is a Design much used in actual experimental work.

Note that we may again find conveniently the differences of $y_{i2k\ell} - y_{i1\ell'}$ $(\ell' = k)$ and call them d or $d' = y_{i2k'\ell} - y_{i1k}$ $(k' \neq k)$ to find least squares estimates of

$$\left.\begin{aligned} \hat{\gamma}_k &= (2\Sigma d_i + \Sigma d_i' - 12\hat{\beta}_2)/3 \\ \hat{\delta}_k &= \Sigma d_i + \Sigma d_i' - 8\hat{\beta}_2 \end{aligned}\right\} \qquad (235)$$

as special cases of Equ. (213). It may even be said that in the model, without consideration of Carry-over, when from Equ. (215)

$$\hat{\gamma}_k = (\Sigma d - \Sigma d')/6 \qquad (236)$$

the structure is so simple that Equ. (176), as in connection with Table XXXII on the 2(3x2x3), can be so simplified.

Breaking up treatment variability - There may easily be a situation in a paired Design, involving more than 2 Treatments, where the variability among Treatments must be broken up, just as it was previously for Youdens in general or for latin squares. An example of such an experiment was provided by the data of Table XXXI on a 9x4x18. The Treatments there consisted of 3 variables each at 2 levels (a high value and a low value). Similarly if one had a study where the technique of trial was by each subject making a single Change-over, but there were involved 9 Treatments the best procedure would be to work with the treatment estimates very much as in the more simple problem of the latin square; one works with totals and then finally adjusts mean squares in allowance for scale. One would start with the treatment estimates and they would be collected, as the nature of the question dictated. To those who have followed the earlier discussion there will be no problem.

Missing data situations on paired Designs - In the extreme situation of but a single Change-over, there can be no question of there being a missing cell. If a participant has only been subject to one Treatment no comparison can be formed for any other Treatment and the single observation is dropped. On the other hand, it may happen that certain pairs are lost and it is impossible to replace them. So in the second sense there may be missing data.

There are, indeed, certain cases where comparisons are deliberately omitted. Thus, if the number of Treatments is very great C_2^t may be excessive and fewer comparisons must be made. Such Designs are discussed by Cox (1958) who gives for 7 Treatments and 4 appearances (replicates he calls them) of each, the Design as follows:

(1) vs. (2)
(2) vs. (3)
(3) vs. (4)
(4) vs. (5)
(5) vs. (6)
(6) vs. (7)
(7) vs. (1)
(1) vs. (3)
(2) vs. (4)
(3) vs. (5)
(4) vs. (6)
(5) vs. (7)
(1) vs. (6)
(2) vs. (7)

If this is considered in the form of a comparison table one gets:

	Treatment						
vs.	(1)	(2)	(3)	(4)	(5)	(6)	(7)
(1)		0	0			0	0
(2)	x		0	0			0
(3)	x	x		0	0		
(4)		x	x		0	0	
(5)			x	x		0	0
(6)	x			x	x		0
(7)	x	x			x	x	

where certain comparisons, such as (1) with (2), are direct and intimate whereas others like (1) with (4) are indirect. This situation is really just a special case of situations discussed in the previous chapter under the topic of Designs more or less Youden.

As a simple, although somewhat trivial, illustration of the situation, there has been formed Table XLIII by simply taking the Design and data for the first 12 Rows of Table XXXVII. In the terms of Cox (1958), this is 6 Treatments in 4 appearances with the additional condition that each Treatment occur twice and twice only in each Column. He was not, of course, concerned with Change-over. It is possible to set up least squares equations, as previously, or else simply to use the program, as in the Appendix. The results are that

$$\begin{aligned}\hat{\mu} &= 73.1/24 \\ &= 3.0 \qquad (237)\end{aligned}$$

and other results are shown in the table.

Table XLIII - Incomplete comparison of 6 Treatments

a. Data

	Period		
Group	1	2	Sum
I	(1) 3.0	(2) 2.7	5.7
II	(2) 3.0	(3) 3.3	6.3
III	(3) 2.6	(4) 3.0	5.6
IV	(4) 3.3	(5) 3.0	6.3
V	(5) 3.1	(6) 2.7	5.8
VI	(6) 3.0	(1) 2.9	5.9
VII	(1) 3.1	(3) 2.8	5.9
VIII	(2) 3.2	(4) 2.7	5.9
IX	(3) 2.1	(5) 2.8	4.9
X	(4) 2.9	(6) 3.4	6.3
XI	(5) 3.5	(1) 3.5	7.0
XII	(6) 3.7	(2) 3.8	7.5
Sum	36.5	36.6	73.1

b. Analysis for Treatments without allowance for Carry-over

	$\hat{(1)}$	$\hat{(2)}$	$\hat{(3)}$	$\hat{(4)}$	$\hat{(5)}$	$\hat{(6)}$
Contr.	+.1	-.0	-.2	-.1	+.2	+.1
Adj. Mn.	3.1	3.0	2.8	2.9	3.2	3.1

Factors	d.f.	Residual Variability (Squares)
$\hat{\mu}$, Rows & Columns (control)	11	.84
Control factors plus Treatments	6	.60

$$F_{5,6} = \frac{(.84 - .60)/5}{.60/6} = .48 \text{ N.S.}$$

c. Analysis for Treatment with allowance for Carry-over

	$(\hat{1})$	$(\hat{2})$	$(\hat{3})$	$(\hat{4})$	$(\hat{5})$	$(\hat{6})$
Contr.	+.4	+.4	+.3	-.7	-.5	+.1
Adj. Mn.	3.4	3.4	3.3	2.3	2.5	3.1

	$[\hat{1}]$	$[\hat{2}]$	$[\hat{3}]$	$[\hat{4}]$	$[\hat{5}]$	$[\hat{6}]$
Contr.	-.3	+.5	+1.4	-.4	-1.0	-.3

Factors	d.f.	Residual Variability (Squares)
$\hat{\mu}$, Rows, Columns & Carries-over (control)	6	.39
Control factors plus Treatments	1	.03

$$F_{5,1} = \frac{(.39 - .03)/5}{.03/1} = 2.12 \text{ N.S.}$$

Although such design gives completely legitimate estimates of treatment effect and of their significance no ingenuity can altogether surmount the various degrees of intimacy with which the comparisons are made. The difficulties show up when one attempts any partition of the variability.

Double reversal trials - Perhaps there should be briefly mentioned a class of Design closely related to those treated in the present chapter, i.e., the double reversal trial. It is used to some extent in Animal Husbandry and is honored by a chapter in Lucas (1969). In the symbolism of the present book it takes the form

	Period		
Cow	1	2	3
I	(1) y_{111}	(2) y_{1221}	(1) y_{1312}
II	(2) y_{212}	(1) y_{2212}	(2) y_{2321}
III	(1) y_{311}	(2) y_{3221}	(1) y_{3312}
IV	(2) y_{412}	(1) y_{4212}	(2) y_{4321}
V	(1) y_{511}	(2) y_{5221}	(1) y_{5312}
VI	(2) y_{612}	(1) y_{6212}	(2) y_{6321}

The study is based essentially on differences of the type $y_{111} + y_{1312} - 2(y_{1221})$. It is argued that insofar as the effect of time is linear such a comparison is free of the effects of Period . Indeed it is. It may further be argued that insofar as such a trend is peculiar to a given unit (Cow) the elimination of trend is similarly peculiar. There is no assumption as in all the work of the present book, that trend is common to all units, or perhaps more fairly that trend, linear or otherwise, is eliminated so far as it is common to all units. The rub remains that the effect of Treatments and Carries-over is completely confounded, so the Design seems ill-suited to our purposes. Furthermore, it does not generalize to Youden rectangles and latin squares.

IX. Epilogue--Misfill

Page

The general problem of misfill 335

Under-fill in two categories 337

Under-fill in more than two categories 352

Over-fill 353

The general problem of misfill - The preceding discussion has frequently skirted problems when in some sense there are missing data. This matter will be now considered extensively. Looking at the matter in a general way, let us call the problem one of misfill. This will include under-fill, as frequently discussed in the literature, over-fill which does not seem to be discussed and a mixture of the two. Fill problems may occur for three reasons. First there is the classical reason that some data have been lost or it has proven impossible to complete the Design. Such a case is illustrated below for the experiment, previously presented, for Oils on a Machine where one of the Machines may be supposed to fail. Secondly, the situation may be such that it is physically impossible to complete the Design. An example, Table XLVIII, is shown where it is impossible (bearing in mind other experimental necessities) to write a Design where Carry-over is properly balanced against Rows. Thirdly, an incorrect fill may be deliberately created because it is more convenient than some other experimental measure. An example previously shown is that of more-or-less Youden situations where a Youden rectangle was deliberately not completed.

The term "fill" is used in the sense of the previous discussion, i.e., the number of times that a Treatment is compared with others in the same Row. Then a misfill will mean that some comparisons are made more or less often than others rather than the same number of times for all comparisons, as in a proper Youden Design. Using the concept in a more extensive way, misfill may be conceived to cover any situation which may be thought of as falling short of some balanced, orthogonal Design.

The discussion in the literature on what is called missing data is what is here termed under-fill. It is mainly, or perhaps exclusively, concerned with a Design where certain scattered cells are missing. In a general way, for any 2-factor problem it is in rows and columns for each of which there occur at least 2 observations. This problem will be considered. In a more extensive and very common way there may be more than 2 factors and whole Rows or Columns may be missing. For instance, a Youden rectangle might be considered perfect if it were as called for, say, in Table XIII. Then the data of Table XXIX is a case of missing data. All 4 observations of the Row

(7) (8) (10) (2) (6)

and of Row

(11) (1) (3) (6) (10)

are, in a sense, missing.

The case of the data of Table XXXIV may be considered as a situation where the patterns

XIV	(1)	(2)	(4)	(7)
XV	(2)	(3)	(5)	(1)
XVII	(4)	(5)	(7)	(3)
XVIII	(6)	(7)	(2)	(5)

are excessive. There if one attempts a single Youden repeated thrice but is unable to complete the 3rd repetition, he may regard the result as an over-fill of a repetition twice or an under-fill of thrice. If, in fact, very little of the 3rd repetition gets done it is natural to fall back on the twice position. Indeed, at times it becomes a matter of view-point whether a table is under-filled or over-filled. Over-fill can be analyzed just as easily as under-fill and by the same general methods. The literature seems to say, however, nothing on the matter.

We can even entertain situations where the number of items is right but they are in the wrong places. Table XLVIII is a situation where such is the case. Most cells are filled once, but some are filled twice and as many not at all. It is, indeed, a comfortable position to regard all problems of the present types as special cases of misfill.

To some extent, the question of misfill is a matter of view-point. The perfect and complete Youden rectangles, themselves, may, as was discussed in Chap. III, on exploration of the Youden field, be regarded as misfilled--incomplete latin squares as it is put in the literature. They do not result in tidy tables with all cells filled and which provide estimates by way of marginal totals. An effort is then made to misfill or under-fill discretely. Whatever the case, it remains an experimental objective to arrange matters so that the categories are as far as possible non-conflicting or orthogonal. If they are not so one cannot distinguish their effects. As it turns out these equations can, as discussed in Chap. V, be very easily solved to give convenient solutions for treatment effects and procedures for analysis that are easily possible of application. The present discussion is concerned with the possibility of solution. A good Design should be as easy as possible of solution.

Under-fill in two categories - Very often some item may be lost in a table of 2-way classification, such as Table XLIV, shown below. It may be said, otherwise, that some cell is missing. Such a situation is often called one of missing data. Let us, however, call it an under-fill. Such situations or problems may occur for three reasons. First there is the classical reason that some data have been lost or it has proven impossible to complete the Design. Such a case is illustrated below in Table XLIV. Secondly, the situation may be such that it is physically impossible to complete the design such as when it is logically or design-wise impossible to get a complete Design for the study of Carry-over. This problem will be discussed in the following chapter on Carry-over. Thirdly, an incorrect fill may be deliberately created because it is more convenient than some other experimental measure. A Design may be almost balanced but a cell or two is missing. Naturally such a Design gives almost the same information as the complete situation but it is difficult of analysis. Regardless of the occasion, we must estimate our effects correctly, i.e., the first power or average effects, regardless of the omissions. In a theoretical way we may have to make an analysis of significance correctly, i.e., do our second power arithmetic of squares, correctly in spite of handicap. With this aspect, however, the present chapter will not concern itself.

Situations, where cells are missing, can be handled by the techniques of missing data that are commonplace in the literature. They, on the whole, are unsatisfactory; apparently the writers have done little to apply them and are unaware of the law efficiency of the methods. Much better can be done and the subject merits a full and general discussion.

The principal practical use of missing data theory, in the context of the present book, is in the forming of unbiased estimates. The determination of significance is little involved. A fairly typical problem is shown in Table XLIV. There is given a statement of satisfaction by each of 11 Men when they were given 6 kinds of Treatment. Each kind was given out to all 11 Men in a given Week. The practical problem is to form an estimate of what degree of satisfaction was gotten for each Product-Week. In this table the confounding of Weeks with Treatments is complete. This situation is, of course, simple because there are then only 2 real categories. Each Week, however, some Men were missing. Accordingly, the observed Means, as shown at the foot of the table were somewhat disturbed by the actual person missing. It is possible to make estimates of Adjusted Means freed of this handicap, as are also shown at the foot of the table. They should, and do, contain less extraneous variability. It can be seen from the above how different are the results with Men missing and with unbiased estimates where allowance is made for those misses.

In a situation such as that of Table XLIV, one might assume a very simple model,

$$y_{ij} = \mu + \alpha_i + \beta_i + \varepsilon_{ij} \qquad (238)$$

where the elements have the same meaning as in earlier, more elaborate cases, and then set up least squares equations as follows:

$$\left.\begin{array}{rrrrrrrrrrrrrrrrrrr}
\hat{\mu} & \hat{\alpha}_1 & \hat{\alpha}_2 & \hat{\alpha}_3 & \hat{\alpha}_4 & \hat{\alpha}_5 & \hat{\alpha}_6 & \hat{\alpha}_7 & \hat{\alpha}_8 & \hat{\alpha}_9 & \hat{\alpha}_{10} & \hat{\alpha}_{11} & \hat{\beta}_1 & \hat{\beta}_2 & \hat{\beta}_3 & \hat{\beta}_4 & \hat{\beta}_5 & \hat{\beta}_6 & = \text{Sum} \\
\hline
55 & 6 & 6 & 3 & 4 & 6 & 6 & 4 & 6 & 6 & 6 & 2 & 9 & 10 & 9 & 8 & 9 & 10 & 142 \\
6 & 6 & & & & & & & & & & & & & & & & & 14 \\
6 & & 6 & & & & & & & & & & & & & & & & 13 \\
3 & & & 3 & & & & & & & & & -1 & & & -1 & -1 & & 13 \\
4 & & & & 4 & & & & & & & & & & -1 & -1 & & & 9 \\
6 & & & & & 6 & & & & & & & & & & & & & 16 \\
6 & & & & & & 6 & & & & & & & & & & & & 11 \\
4 & & & & & & & 4 & & & & & & & & & -1 & -1 & 8 \\
6 & & & & & & & & 6 & & & & & & & & & & 22 \\
6 & & & & & & & & & 6 & & & & & & & & & 18 \\
6 & & & & & & & & & & 6 & & & & & & & & 12 \\
1 & 1 & 1 & 1 & 1 & 1 & 1 & 1 & 1 & 1 & 1 & 1 & & & & & & & 0 \\
9 & & & -1 & & & & & & & & -1 & 9 & & & & & & 26 \\
10 & & & & & & & & & & & -1 & & 10 & & & & & 25 \\
9 & & & & -1 & & & & & & & -1 & & & 9 & & & & 26 \\
8 & & & -1 & -1 & & & & & & & -1 & & & & 8 & & & 19 \\
9 & & & -1 & & & & -1 & & & & & & & & & 9 & & 26 \\
10 & & & & & & & -1 & & & & & 1 & 1 & 1 & 1 & 1 & 1 & 0
\end{array}\right\}\quad(239)$$

(The positions, left empty, contain zeroes, not shown)

It is, however, generally more practical to use a simple iterative approach. The one recommended is shown in detail in Table XLIV. This iterative procedure is algebraically equivalent to setting up least squares equations for $\hat{\mu}$ and the two categories are solving. It is, however, often convenient to carry out this simple procedure on a desk calculator. Even if it is assigned to an electronic computer, it may be quicker and cheaper to mechanize the iteration than to have the machine set up equations and then solve.

In the calculation of Table XLIV, all that is involved is essentially the iterative solution of the 3 equations for μ , Men and Product-Week. As the problem was actually worked the steps were as follows:

a. Form means of Rows. For Row III, thus, get 13/3 = 4.3. For full Rows the means are also needed.

b. Form the right-hand corner mean as the mean of the Row means, including the complete Rows.

c. Find the contribution of each Row (only those incomplete are needed). Thus for Row III, the contribution is 4.3 - 2.7 = + 1.6.

d. Form adjusted Column sums by adding for each the contributions of the missing Rows. Thus for Column (1) get 26 + 1.6 + .3 = 27.9. Divide this adjusted sum by the number of items actually in the Column to get adjusted mean. Thus for Column (1) get 27.9/9 = 3.1.

e. Form new adjusted corner mean as the mean of the Row means, including any complete Columns.

f. Find the contribution of each Column (only those incomplete are needed). Thus for Column (1), the contribution is 3.1 - 2.7 = + .4.

g. Form second adjusted Row sum by adding for each the contribution of the missing Columns. Thus for Row III get 13 + .4 - .1 + .3 = 13.6. Divide this adjusted sum by the number of items actually in the Row to get adjusted mean. Thus for Row III get 13.6/3 = 4.5.

h. Form new adjusted corner mean as the mean of 2nd adjusted Row means, including any complete Rows.

i. Find the contributions of each Row (only those incomplete are needed). Thus for Row III, the contribution is 4.5 - 2.7 = + 1.8.

j. Return to step d. Continue moving from Rows to Columns to Rows etc. until the adjusted mean is unchanged from the last step.

k. The Column means, which are the result wanted in the present case are then read from the foot of the total.

Table XLIV - Satisfaction by Men and Treatments with missing cells

Recommended iterative solution

	Treatment									Adj.	2nd	Adj.
Man	(1)	(2)	(3)	(4)	(5)	(6)	Sum	Mn.	Contr.	Mn.	Contr.	Mn.
I	3	1	3	3	2	2	14	2.3				
II	2	2	2	2	2	3	13	2.2				
III	NR	5	4	NR	NR	4	13	4.3	+1.6	4.5	+1.8	4.5
IV	3	1	NR	NR	3	2	9	2.2	- .5	2.3	- .4	2.3
V	4	3	2	3	4	0	16	2.7				
VI	2	1	2	2	2	2	11	1.8				
VII	2	2	2	2	NR	NR	8	2.0	- .7	1.9	- .8	1.9
VIII	4	5	5	3	5	0	22	3.7				
IX	3	3	3	3	3	3	18	3.0				
X	3	2	3	1	2	1	12	2.0				
XI	NR	NR	NR	NR	3	3	6	3.0	+ .3	3.2	+ .5	3.2
Mean	2.9	2.5	2.9	2.4	2.9	2.0						
Sum	26	25	26	19	26	20	142					
Adj.Mn	3.1	2.5	2.9	2.6	3.0	1.9		2.7				
Contr.	+.4	-.2	+.2	-.1	+.3	-.8						
Adj.Mn	3.1	2.6	2.9	2.6	3.0	1.9				2.7		
Contr.	+.4	-.1	+.2	-.1	+.3	-.8						

The procedure usually recommended in the literature for this situation involves filling the missing cells with estimates of what the observations might have been and then proceeding to the formation of Row and Column means and contributions. The details of calculation are shown in Table XLV. There the cells of missing data are filled with italicized values which are the product of calculation, as immediately below. The essential difference between these two procedures is that in Table XLIV the effort is concentrated on the formation of corner, row and column estimates, whereas in Table XLV it is on the formation of cell estimates. From Table XLIV the cell estimates could be found as a by-product while in Table XLV the row and column estimates are found as a by-product.

The steps in calculating Table XLV are as follows:

a. Fill in each missing cell with some guess as to its proper value. These guesses may be a single number or various numbers. For the sake of illustration suppose they are all filled in with the observed overall mean 142/55 = 2.6.

b. Form row, column and corner means. For corner mean get {142 + 11(2.6)}/66 = 2.6. For Row III get {13 + 3(2.6)}/6 = 3.5. For this Row the contribution is 3.5 - 2.6 = + .9. For Column 1 get {26 + 2 (2.6)}/11 = 2.8. For this Column the contribution is 2.8 - 2.6 = + .2.

c. Reform the estimate of missing values as corner average plus row and column contributions. Thus for the missing value in Row III and Column 1 , get 2.7 + .9 + .2 = 3.8.

d. When all 11 missing values have been so reformed, repeat the operation to find that now the contribution of Row III is + 1.4 (as in Table XLV), for Column 1 is + .4 and these superimposed upon a corner mean which is still 2.6 gives a second reformed value for the first missing value of 2.6 + 1.4 - .4 = 3.6. So proceed until the matter settles down.

Table XLV - Satisfaction by Men and Treatments with missing cells - Iterative solution, not recommended

	Week							1st		2nd		3rd		4th		5th		
Man	(1)	(2)	(3)	(4)	(5)	(6)	Sum	Adj. Mn.	Con-tr.	Adj. Mn.	Con-tr.	Adj. Mn.	Con-tr.	Adj. Mn.	Con-tr.	Adj. Mn.	Con-tr.	Adj. Mn.
I	3	1	3	3	2	2	14	2.3										
II	2	2	2	2	2	3	13	2.2										
III	4.8	5	4	4.3	4.7	4	13	3.5	+.9	4.0	+1.4	4.3	+1.7	4.4	+1.7	4.5	+1.8	4.5
IV	3	1	2.5	2.2	3	2	9	2.4	-.2	2.3	- .3	2.3	- .3	2.3	- .4	2.3	- .4	2.3
V	4	3	2	3	4	0	16	2.7										
VI	2	1	2	2	2	2	11	1.8										
VII	2	2	2	2	2.3	1.2	8	2.2	-.4	2.0	- .6	2.0	- .6	2.0	- .7	1.9	- .8	1.9
VIII	4	5	5	3	5	0	22	3.7										
IX	3	3	3	3	3	3	18	3.0										
X	3	2	3	1	2	1	12	2.0										
XI	3.5	2.9	3.3	3.0	3	3	6	2.7	+.1	2.8	+ .2	3.0	+ .4	3.1	+ .4	3.1	+ .4	3.1
Sum	26	25	26	19	26	20	142											
Adj.Mn	2.8	2.5	2.8	2.4	2.8	2.1		2.6										
Contr	+.2	-.1	+.2	-.2	+.2	-.5												
Adj.Mn	3.0	2.5	2.9	2.5	2.9	2.0				2.6								
Contr	+.4	-.1	+.3	-.1	+.3	-.6												
Adj.Mn	3.1	2.5	2.9	2.5	3.0	1.9						2.6						
Contr	+.5	-.1	+.3	-.1	+.4	-.7												
dj.Mn	3.1	2.5	2.9	2.6	3.0	1.9								2.7				
ontr	+.4	-.2	+.2	-.1	+.3	-.8												
dj.Mn	3.1	2.5	2.9	2.6	3.0	1.9										2.7		
ontr	+.4	-.2	+.2	-.1	+.3	-.8												
dj.Mn	3.1	2.5	2.9	2.6	3.0	1.9												2.7

It may be observed that this method takes 5 iterations in contrast to the 2 iterations of Table XLIV. Furthermore, the formation of the cell values is very troublesome, so that there is almost twice the work for a given iteration. All in all, the work of Table XLV is about 10 times that of Table XLIV and such a ratio is fairly typical. It will be noted that the results in the two tables differ slightly and this because those in Table XLV are, within the bounds of rounding error, dependent on the number, or numbers, with which the missing cells are filled at first approximation. If we had started by filling each with a large number, like 5.0 instead of 2.6, the contributions in Table XLV would have been a little higher. We can only sum up the procedure of filling missing cells by saying that an extremely heavy sacrifice is made for the sake of the orthogonal point of view. The filling is essentially done to make the data orthogonal. This is of a piece with the forcing of tests of significance into the analysis of variance.

It is, of course, possible to test significance of the treatment effects in Table XLIV; it is indeed commonly done in the literature. Such experiments are, however, somewhat outside the field of the present book and so the matter will not be pursued further.

One place where under-fill in two categories arises very naturally and is employed frequently, in work of the present kind, is in the quick examination of data to see whether there is any indication of Carry-over. For example, the latin square of Table XXIV may be examined in this way as in Table XLVI. Data are arranged according to Treatment and preceding Treatment so that averages may be found quickly. In this Design, with a conditioning Period, the only complication arises from the missing Row of the perfect Design. It is perhaps wisest to find adjusted means by iteration. They are plainly a little different from the simple means. All such analysis is, of course, incomplete because no allowance is made for the slight confounding of Carry-over with Row.

Note that $\hat{\mu}$ is subject to constant estimation as the iteration proceeds, because the losses are identified with certain Treatments and Carries-over. It may be estimated as the average of treatment averages or, at an appropriate stage, of carry-over averages.

In case the calculation of Table XLVI is not immediately apparent, it may be sketched, first, from the after means 51.4, 54.1 etc.; the first estimate of μ is their average, i.e., 55.5. The first estimates of after contributions are 51.4 - 55.5 = -4.1 etc. The latter are used to adjust the treatment totals. Thus for Treatment (1) the sum becomes 264.8 + (- 1.4) = 263.4. The first estimate of mean for this Treatment then is 263.4/5 = 52.7. The estimate of μ as the average of the values 52.7, 51.0 etc. happens to remain at 55.5. The contributions are 52.7 - 55.5 = - 2.8 etc. The latter values are now used to adjust the after averages. Thus after (2) becomes {270.6 + (- 2.8)}/5 = 53.6. The business is pursued until contributions for either After or Treatment stabilize. Note that the actual means for Treatments have been added for the sake of comparison.

Table XLVI - Data from Table XXIV, an incomplete latin square, with conditioning Period examined quickly for evidence of Carry-over

	Treatment									2nd		3rd		
After	(1)	(2)	(3)	(4)	(5)	(6)	Sum	Mn.	Contr	Adj. Mn.	Contr	Adj. Mn.	Contr	Adj. Mn.
(1)	46.4	45.8	58.4	52.0	56.4	49.6	308.6	51.4	-4.1	51.4	-4.2	51.4	-4.2	51.4
(2)		50.0	40.8	50.0	64.2	65.6	270.6	54.1	-1.4	53.6	-2.0	53.5	-2.1	53.5
(3)	51.8		60.9	62.4	53.3	59.2	287.6	57.5	+2.0	56.6	+1.0	56.6	+1.0	56.6
(4)	50.4	53.6		48.8	59.9	64.2	276.9	55.4	- .1	55.6	.0	55.6	.0	55.6
(5)	55.3	58.9	58.7		63.2	61.7	297.8	59.6	+4.1	59.1	+3.5	59.1	+3.5	59.1
(6)	60.9	44.9	64.2	49.6			219.6	54.9	- .6	57.0	+1.4	57.1	+1.5	57.1
Mean	53.0	50.6	56.6	52.6	59.4	60.1								
Sum	264.8	253.2	283.0	262.8	297.0	300.3	1661.1							
Adj.Mn	52.7	51.0	56.6	53.4	59.3	59.9		55.5						
Contr	-2.8	-4.5	+1.1	-2.1	+3.8	+4.4								
Adj.Mn	52.6	50.8	56.6	53.3	59.7	60.3				55.6				
Contr	-3.0	-4.8	+1.0	-2.3	+4.1	+4.7								
Adj.Mn	52.5	50.8	56.6	53.3	59.7	60.4						55.6		
Contr	-3.1	-4.8	+1.0	-2.3	+4.1	+4.8								

As another example of an under-fill situation that arises in considering results by Treatment and preceding Treatment, there are the data of Table XXIII, as examined in Table XLVII. In this Design, without a conditioning Period, the cells on the principal diagonal are necessarily unfilled and it is again wisest to find adjusted mean by iteration. They are plainly a little different from the simple means. This under-fill arises not from some data having been somehow lost or ungotten, but rather from the logical difficulties of the Design. In such tables, where the results are classified by Treatment and after Treatment, one has in a very direct way simply an example of incomplete fill.

An important point that seems difficult is that horizontally the last line is not involved but vertically it is. This may be called the mixed model that arises in unconditioned experiments. Accordingly, the quantity on which one is zero-centering varies according to the direction of interest. The value, be it the 52.0 horizontally, or the 53.0 vertically is set once and for all as the average of appropriate observations, because the omission of direct Treatment effects and of Carries-over is that of the complete set.

In case the calculation of Table XLVII is not immediately apparent, it may be sketched. First, form the after means 53.3, 53.9 etc. In order to zero-center them, find their average of 52.0. The first estimates of after contributions are 53.3 - 52.0 = + 1.3 etc. The latter are used to adjust the treatment totals (which include the data of the first experimental Period, i.e., background observations 56.8, 57.8 etc.). Thus for Treatment (1) the mean becomes {208.0 + (+ 1.3)}/4 = 52.3. In order to zero-center them, find their average which is now 53.0. The contributions are 52.3 - 53.0 = - .7 etc. The latter values are now used to adjust the after averages. Thus after (1) becomes {159.8 + (- .7)}/3 = 53.0. Such values are again zero-centered. Note that the actual means for Treatments have been added for the sake of comparison.

Table XLVII - Data of Table XXIII examined quickly for evidence of Carry-over in a latin square without conditioning Period

	Treatment							2nd		
After	(1)	(2)	(3)	(4)	Sum	Mn.	Contr	Adj. Mn.	Contr	Adj. Mn.
(1)		57.2	54.0	48.6	159.8	53.3	+1.3	53.0	+1.0	53.0
(2)	52.2		54.6	54.8	161.6	53.9	+1.9	54.3	+2.3	54.3
(3)	47.0	50.6		53.0	150.6	50.2	-1.8	50.6	-1.4	50.6
(4)	52.0	49.4	50.6		152.0	50.6	-1.4	50.1	-1.9	50.1
Bk.Gr.	56.8	57.8	59.2	50.4	224.2					
Mean	52.0	53.8	54.6	51.7						
Sum	208.0	215.0	218.4	206.8	848.2					
Adj.Mn	52.3	54.2	54.2	51.4		52.0				
Contr	- .7	+1.2	+1.2	-1.6		53.0				
Adj.Mn	52.2	54.3	54.2	51.2				52.0		
Contr	-.8	+1.3	+1.2	-1.8				53.0		

The technique just illustrated in Table XLVII works ill for Youdens, particularly if c is much less than t . Such a case was that shown in Table XXIId. In the first place the iteration is very slow because the table is so incompletely filled and in the second place because not only is Carry-over confounded with Row but it is confounded with the same Row as its Treatment, counterpart.

Under-fill in more than two categories - Problems, analogous to those of Tables XLIV through XLVII, i.e., under-filled situations, are in 3 categories, say Men, Periods and Treatments, for complete and satisfactory solution, if one is simply after the analysis for direct treatment effects. The situation with 2 cells missing as previously shown in Table XXV, for example, cannot conveniently be handled by forming marginal totals, adjusted totals etc. with Rows, Columns and Treatments to consider. This because we lack

3-dimensional paper. It is perhaps best to do it essentially by setting up the least squares equations and solving them iteratively, if it must be done on a desk calculator, but it does not seem worthwhile to illustrate the matter. If one is after Carry-over also, 4 categories are involved. If one so cared,he might set up the various equations and solve them iteratively. We used to do such things but now rejoice in the progress of our times--it was dreadful. When the Design is incomplete by certain full Rows the procedure is exactly the same; the programs in the Appendix may be used.

As one of the many examples of the type of problem involved there are the data of Table XXIII. As can be seen from the rough analysis as in Table XXIIIb the estimation for Carry-over is complicated by the fact that the Design is incompletely filled with observations. Let us say that the Design is essentially a matter of effects of various kinds such as direct treatment effects and Carry-over, appearing an equal number of times in all combinations but for various practical reasons, this objective is not attained.

The procedure, just indicated, also becomes intolerable if one attempts to extend it to a Youden rectangle $c < t$ with or without Carry-over.

Over-fill - We shall encounter the problem of over-fill as well as missing data or under-fill, although the literature seems silent on the subject. No doubt such may happen in many ways or many situations may be so viewed. Let us confine our attention to a case when this is not due to accident but exigencies of design. The over-fill is not to be due to accident but to the fact that it is logically impossible to write a Design when the cells are evenly filled. An example is provided by the consideration of

Carry-over effect against Blocks in the data of Table VI, as shown in Table XLVIII. This is an entirely legitimate question because Treatments are orthogonal to both other effects and we might choose to clear them of one another. The results would be final and unembarrassed. It will be seen that on the principal diagonal the positions are double-filled, while on the secondary diagonal they are empty. One might say so or say that the table is filled twice although in most cells one observation is missing and in some both. Thus while the contribution of Carry-over [6] may be influenced one way because Group I is missing it may also be affected because Group VI is present twice. We may say the Design is under-filled (missing plot) or over-filled or in general, misfilled. Both under- and over-filling aspects occur in this single experiment.

The required estimates can be gotten by an iterative technique similar to that recommended for under-filled or missing data situations. The sum for each Row or Column, successively, is adjusted for the contributions that it lacks but also now for those that it has excessively, an average struck and contributions found. One comes out with adjusted averages for Groups and Carry-over. In the present case they are changed from the direct averages that one gets. The details of iteration are as follows. They are started by finding the simple row means, the corner mean 56.4 and from these a just approximation to the row contributions. The first adjusted column means were found by making allowance for these first row contributions. Thus the mean for Carry-over [1] is $\{308.6 - (- 3.6) + 5.4\}/6 = 52.9$. The corner average remains unchanged in a perfectly balanced misfill of this kind. The column contributions were then found and used to get a second estimate of row mean and contribution. Thus for Group I the estimate becomes {317.0 - (- 3.5) + 1.3}/6 = 536. Means stabilize by the 3rd iteration of the row means. Of course, in practice one does not usually write down the successive iterations but simply writes them in pencil and uses india rubber.

If one regards a missing data situation as a matter of trying to replace the bit of data, one cannot handle the idea of over-fill at all. One would have to fall back on the concept of the table's being filled twice in each cell but with half the values, i.e., 36 of them, missing. One would have to put in 36 estimates, strike averages, return to improve the 36 estimates etc. The operation would be monstrous.

Table XLVIII - Carry-over against Group from the data of Table VI, an example of over-fill

			Carry-over								2nd		3rd	
Group	[1]	[2]	[3]	[4]	[5]	[6]	Sum	Mn.	Contr	Adj. Mn.	Contr	Adj. Mn.	Contr	Adj. Mn.
I	46.4 45.8	40.8	62.4	59.9	61.7		317.0	52.8	-3.6	53.6	-2.8	53.7	-2.7	53.7
II	58.4	50.0 50.0	53.3	64.2		60.9	336.8	56.1	- .3	57.5	+1.1	57.7	+1.3	57.7
III	52.0	64.2	60.9 59.2		55.3	44.9	336.5	56.1	- .3	55.8	- .6	55.8	- .6	55.8
IV	56.4	65.6		48.8 50.4	58.9	64.2	344.3	57.4	+1.0	57.6	+1.2	57.7	+1.3	57.7
V	49.6		51.8	53.6	63.2 58.7	49.6	326.5	54.4	-2.0	53.0	-3.4	52.9	-3.5	52.9
VI		52.7	53.3	57.3	71.7	63.7 72.0	370.7	61.8	+5.4	61.0	+4.6	60.9	+4.5	60.9
Mean Sum	51.4 308.6	53.9 323.3	56.8 340.9	55.7 334.2	61.6 369.5	59.2 355.3	2031.8	56.4						
Adj.Mn Contr	52.9 -3.5	53.6 -2.8	57.0 + .6	55.5 - .9	61.9 +5.5	57.7 +1.3								
Adj.Mn Contr	52.7 -3.7	53.1 -3.3	57.1 + .7	55.4 -1.0	62.3 +5.9	58.0 +1.6								
Adj.Mn Contr	52.6 -3.8	53.1 -3.3	57.1 + .7	55.4 -1.0	62.4 +6.0	58.0 +1.6								

Acknowledgements - It was possible to complete this work on account of the generosity of Mr. Harry H. Harman who made available to me, as a Visiting Scholar, the facilities of the Educational Testing Service. His successor, Dr. A. E. Beaton, continued that hospitality. I am profoundly grateful.

To two colleagues I owe much. Dr. C. H. Eaton first taught me how to handle change-over experiments. Mr. Bary G. Wingersky, discussed the work and cleared up points, starting in 1946 and continuing until 1970.

Many assistants helped in the detail and guided my thinking. The late, good Geraldine Davey shaped up the Designs in the first place. Siu-shyong Lin helped vastly in writing the many new Designs found. Virginia Halfmann wrote the electronic programs that follow and that were used to write out in final form the many analyses. J. J. Ferris continued support in computing when Miss Halfmann left. He also contributed to the argument as may be seen from some of the references.

To my secretary, Mrs. Beatrice Stricklin, what can I say? She patiently typed and retyped, checked and rechecked. But beyond that she made herself responsible for the form of much of the work. Her strict attention discovered many errors. A good woman is beyond the price of rubies.

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[*] The material is available to the public but seems to be an open mimeograph volume dated from the year of its purchase.

Table L. Variance ratio for 5%, 1% and .1% levels according to freedom of denominator and numerator.

This table might be shown if the present work is printed. For the moment, the reader can refer to many books, notably Fisher and Yates (1967) or Cochran and Cox (1957).

Appendix - Programs for electronic computers

There follow two programs for the electronic computer. The first is for an experiment without conditioning Period, i.e., one where the first Period of experimental results is the first Period of Treatment. The second is for an experiment with a conditioning Period, i.e., one where the first Period of experimental results is preceded by a Period to control the Change-over. Many people will never need the second program. These programs make an analysis, along the lines discussed in the present book, given the Design and results for an experiment. It is convenient, and in some cases vital, for the actual analyses discussed in the text. It is written in FORTRAN 4, as used by the stand-apart, IBM 360-65 at Educational Testing Service in 1969. This program is accompanied by a discussion of the steps of its construction with the idea that it may be modified a little for other similar machines or for a shared-time system. The latter is, of course, much preferable for problems of the magnitude usual in experiments like those discussed in the present book.

In these programs all Rows must be entire. Shortcomings in the Design, i.e., missing Rows do not affect the execution. The programs deal with pairs or a single Change-over up to Designs for latin squares. The analysis is made without allowance for Carry-over and if possible with that. In the more simple first alternative it is suitable for experiments that are not change-over experiments. Various size limits to number of Treatments, etc. are suggested in the programs. They depend upon the storage space in DIMENSION. If this is exceeded the machine will give unreasonable results and may stop.

When one looks at it closely one sees that it is concerned entirely with the business of setting up the necessary least squares equations. This is the skeleton of the problem and once this is done out, we can hang all the organs about, like necessary conditions, residuals squares and print-out.

The program will accept and handle paired comparisons, i.e., those with but a single Change-over. A reduced form of this program suitable only for paired comparisons, without conditioning might be written by simply cutting down the present program.

The detailed discussion of the program that follows is a reprint[*] of program #1.2 of the Manual of Scientific Programs, Office of Computation Sciences, Educational Testing Service, Princeton, N.J.

I. The class of designs covered is latin squares or Youden rectangles (incomplete latin squares). These may be repeated fully or in part. The design may be defective, i.e., certain whole rows may be missing, but no allowance has been made for missing cells, i.e., single observations.

The purpose of this program is primarily to analyze the results for effect of Treatment with allowance for Carry-over of preceding Treatment. There is also direct testing of the significance of Carry-over. There is included parallel estimation and testing of significance without allowance for Carry-over.

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[*] By kind permission of the Director, Mr. Harry H. Harman.

The program, as presently stored, allows analysis for designs up to 40 Rows (or blocks). This is limited by the dimension of the data matrices. It could be readily enough changed to 500 or 1000, if such an experiment were involved. The analysis has been contrived so that such increase does not increase the size of the matrix involved in equation solving.

II. GENERAL

The basic equations are

$$(1) \qquad y_{ijk} = \mu + \alpha_i + \beta_j + \gamma_k + \varepsilon_{ijk}$$

where y_{ijk} is an observation assumed built of a general level μ, effect of the i^{th} Row or individual α_i, the j^{th} Period or Column β_j, the k^{th} Treatment γ_k and extraneous variability ε_{ijk}. This equation obtains for the first Column or Period when there has been no conditioning Period. For the following Periods

$$(2) \qquad y_{ijk\ell} = \mu + \alpha_i + \beta_j + \gamma_k + \delta_\ell + \varepsilon_{ijk\ell}$$

where the effect of the ℓ^{th} Treatment in the preceding Period is δ_ℓ. For a conditioned experiment Equ. (2) obtains in all Columns.

The data actually considered are the differences within Rows such as

$$(3) \qquad y_{ijk\ell} - y_{i'j'k'\ell'} = \beta_j - \beta_{j'} + \gamma_k - \gamma_{k'} + \delta_\ell - \delta_{\ell'}$$

$$(j' \neq j \,,\; k' \neq k \,,\; \ell' \neq \ell) \;.$$

These differences are then set forth in a matrix. Thus for an unconditioned latin square for which the first line is

Design	(1)	(2)	(4)	(3)
Result	4	5	7	6

we may consider the two differences

$$(4)\quad \begin{aligned} y_{111} - y_{1221} &= \beta_1 - \beta_2 + \gamma_1 - \gamma_2 - \delta_1 + \varepsilon_{111} - \varepsilon_{111} \\ y_{1221} - y_{1342} &= \beta_2 - \beta_3 + \gamma_2 - \gamma_4 + \delta_1 - \delta_2 + \varepsilon_{1221} - \varepsilon_{1342} \end{aligned}$$

which results in two lines of the matrix as follows:

β_1	β_2	β_3	β_4	γ_1	γ_2	γ_3	γ_4	δ_1	δ_2	δ_3	δ_4	Result
+1	-1			+1	-1			-1				-1
	+1	-1			+1		-1	+1	-1			-2

Least squares equations are in the same form. For instance, to get the equation associated with β_2 each line is multiplied by its content in the β_2 column and the product accumulated over all columns. For each set of effects (β , γ or δ) the last equation is replaced by a condition equation

$$(5)\qquad \sum_j \hat{\beta}_j = \sum_k \hat{\gamma}_k = \sum_\ell \hat{\delta}_\ell = 0 \quad .$$

The analysis without Carry-over (SANS DELTA) is gotten by replacing, temporarily, all equations appropriate to δ by

$$(6)\qquad \hat{\delta}_\ell = 0 \quad .$$

The analysis without Treatment (SANS GAMMA) is obtained by the temporary replacement

$$(7)\qquad \hat{\gamma}_k = 0 \quad .$$

The residual variability is gotten in several steps. First the variability residual on β , γ and δ is

$$(8)\qquad \sum_{ijk} y^2_{ijk} + \sum_{ijk\ell} y^2_{ijk\ell} - \sum_j \hat{\beta}_j \Big(\sum_{ik} y_{ijk} + \sum_{ik\ell} y_{ijk\ell}\Big) - \sum_k \hat{\gamma}_k \Big(\sum_{ij} y_{ijk} + \sum_{ij\ell} y_{ijk\ell}\Big) - \sum_\ell \hat{\delta}_\ell \sum_{ijk} y_{ijk\ell} \quad .$$

Secondly, an estimate of $\hat{\mu}$ is made by finding from Equ. (1) or (2) the mean of the values

$$y'_{ijk} = y_{ijk} - \hat{\beta}_j - \hat{\gamma}_k$$

(9)

$$y'_{ijk\ell} = y_{ijk\ell} - \hat{\beta}_j - \hat{\gamma}_k - \hat{\delta}_\ell$$

Thirdly, estimates of $\hat{\alpha}_i$ are made by finding the mean of the values

$$y''_{ijk} = y'_{ijk} - \hat{\mu}$$

(10)

$$y''_{ijk\ell} = y'_{ijk\ell} - \hat{\mu} \quad .$$

The residual variability as from equation (8) is then further and finally reduced by

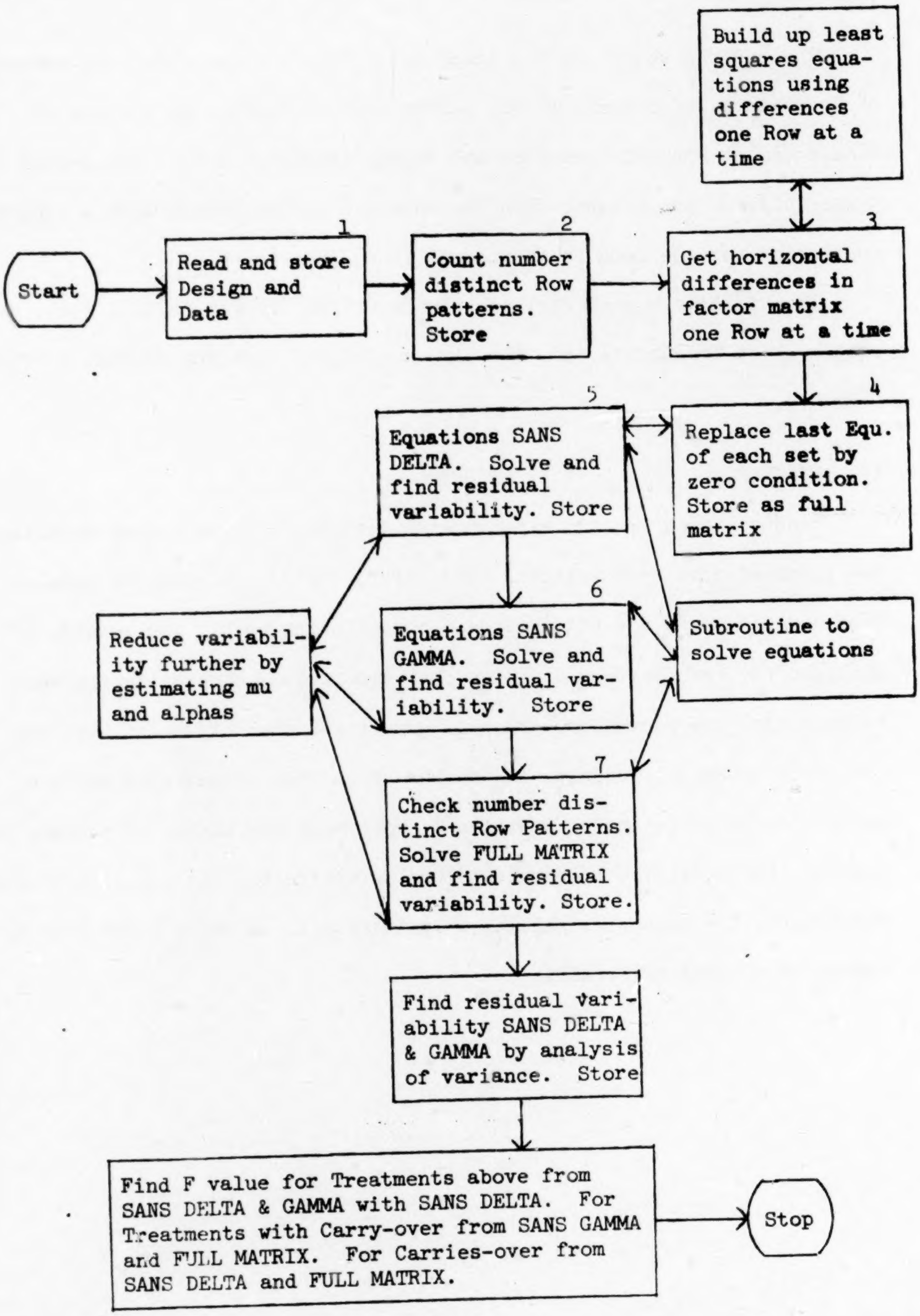
$$\hat{\mu} \sum_{ijk\ell} y_{ijk\ell} + \sum_i \alpha_i \sum_{jk\ell} y_{ijk\ell} \quad .$$

Residual variability on the effects of μ , Rows and Columns only (SANS DELTA & SANS GAMMA) is gotten by the formula familiar in analysis of variance.

The test of significance for Treatments without allowance for Carry-over is based on residual variability SANS DELTA less residual variability SANS DELTA & SANS GAMMA. The test with allowance for Carry-over is from residual variability on FULL MATRIX less that on SANS GAMMA. The test for Carry-over is from residual variability on FULL MATRIX less that on SANS DELTA.

It need only be added that there is incorporated a test on whether the situation is under-determined. The program counts the number of different Row patterns, multiplies this number by the number of Columns and checks whether the result exceeds the number of independent parameters to be estimated. In the case of under-determination it refuses to analyze. A second type of refusal arises if the simultaneous equations prove insoluble, which may arise if the design is redundant. Finally, if there is no residual freedom, the program will estimate the parameters but declare $F = 0$.

The scheme is diagramatically as follows:


Start
1
Read and store Design and Data
2
Count number distinct Row patterns. Store
3
Get horizontal differences in factor matrix one Row at a time
Build up least squares equations using differences one Row at a time
4
Replace last Equ. of each set by zero condition. Store as full matrix
5
Equations SANS DELTA. Solve and find residual variability. Store
6
Equations SANS GAMMA. Solve and find residual variability. Store
Subroutine to solve equations
Reduce variability further by estimating mu and alphas
7
Check number distinct Row Patterns. Solve FULL MATRIX and find residual variability. Store.
Find residual variability SANS DELTA & GAMMA by analysis of variance. Store
Find F value for Treatments above from SANS DELTA & GAMMA with SANS DELTA. For Treatments with Carry-over from SANS GAMMA and FULL MATRIX. For Carries-over from SANS DELTA and FULL MATRIX.
Stop

III. INPUT

The program starts with a parameter card which indicates the numbers of Treatments, of Columns in the pattern of Treatments, of Columns of Result and of Rows of Treatment and Result (FORMAT (4I5)). The second number will be one greater than the third in an experiment with a conditioning Period but the same in an unconditioned experiment.

Then follows a card for each Row of Design (FORMAT (6I5)), i.e., statement of Treatments and, finally, a card for each Row of Result (FORMAT (6F5.0)).

IV. PROGRAM

Because there are two experimental designs for change-over experiments, two programs have been written. The first, ZFE-03, analyzes the results from a design that did not include a conditioning week. The second, ZFE-04, analyzes the results from a design that did include a conditioning week. In each case the parameter card will give the number of treatments, the number of columns of result, the number of columns of patterns and the number of rows. In the unconditioned experiment the number of columns of results will equal the number of columns of patterns. In the conditioned experiment, the number of columns of pattern will be one greater than the number of columns of results.

V. OUTPUT

The Design and Results are printed out. The least squares estimates for Columns and Treatments are printed (SANS DELTA) as they are found without consideration of Carry-over. Then the estimates for Columns, Treatments and Carries-over are shown (FULL MATRIX). Finally the analyses for significance are printed. There is a test for Treatment, without allowance for Carry-over, a test for Treatment, with allowance, and a test for Carry-over. In each case there is shown the variance ratio, F , with its two statements of freedom.

VI. ILLUSTRATIVE EXAMPLE

By way of illustration there follows the case of a double Youden rectangle for 7 Treatments and 14 Rows. Six Rows of the Design were repeated but in the end 2 men were lost so that there were 18 Participants.

```
PARAMETER CARD
   7      4      4    18
PATTERNS                          RESULTS
   1      2      4     7             55.00    37.00    47.00    45.00
   2      3      5     1             37.00    25.00    25.00    38.00
   4      5      7     3             24.00    24.00    29.00    46.00
   5      6      1     4             36.00    44.00    39.00    52.00
   6      7      2     5             42.00    54.00    50.00    55.00
   7      1      3     6             49.00    37.00    56.00    62.00
   1      5      3     2             42.00    45.00    35.00    32.00
   2      6      4     3             55.00    60.00    52.00    60.00
   3      7      5     4             54.00    60.00    57.00    54.00
   4      1      6     5             42.00    44.00    46.00    44.00
   5      2      7     6             46.00    48.00    54.00    47.00
   6      3      1     7             42.00    54.00    42.00    42.00
   7      4      2     1             49.00    42.00    47.00    48.00
   1      2      4     7             34.00    43.00    46.00    54.00
   2      3      5     1             52.00    55.00    42.00    42.00
   3      4      6     2             38.00    38.00    26.00    28.00
   4      5      7     3             50.00    62.00    50.00    61.00
   6      7      2     5             42.00    49.00    51.00    63.00


              ESTIMATES OF VARIABLES

SANS DELTA                                          FULL MATRIX


                          BETA
      -1.3858                                           -1.3742
      -0.1037                                           -0.1668
      -1.3409                                           -1.3384
       2.8304                                            2.8795
                          GAMMA
      -1.9907                                           -1.4136
      -1.1776                                           -0.5279
       4.1289                                            3.8279
      -0.6008                                           -0.9817
       0.1367                                           -0.6040
      -1.2811                                           -1.4894
       0.7846                                            1.1887
                          DELTA
      -0.0                                               0.7361
      -0.0                                               2.8368
      -0.0                                               0.7053
      -0.0                                              -0.1414
      -0.0                                              -2.0391
      -0.0                                              -1.4626
       0.0                                              -0.6351
```

```
ANALYSIS

                  FACTORS                    | D.F. |  RESIDUAL VAR
---------------------------------------------+------+---------------
MU ROWS COLUMNS                     CONTROL  | 51.  |      2188.37
CONTROL FACTORS PLUS     TREATMENTS          | 45.  |      1973.20
---------------------------------------------+------+---------------


  F          = (   2188.37 -   1973.20 )/ 6.   =    0.82
     6.,45.              1973.20 / 45.

                  FACTORS                    | D.F. |  RESIDUAL VAR.
---------------------------------------------+------+---------------
MU ROWS COLUMNS CARRIES-OVER CONTROL         | 45.  |      2045.65
CONTROL FACTORS PLUS     TREATMENTS          | 39.  |      1874.19
---------------------------------------------+------+---------------


  F          = (   2045.65 -   1874.19 )/ 6.   =    0.59
     6.,39.              1874.19 / 39.

                  FACTORS                    | D.F. |  RESIDUAL VAR.
---------------------------------------------+------+---------------
MU ROWS COLUMNS TREATMENTS   CONTROL         | 45.  |      1973.20
CONTROL FACTORS PLUS     CARRIES-OVER        | 39.  |      1874.19
---------------------------------------------+------+---------------


  F          = (   1973.20 -   1874.19 )/ 6.   =    0.34
     6.,39.              1874.19 / 39.
```

There follows the program for an experiment without conditioning Period, i.e., one where the first Period of experimental results is the first Period of Treatment. There is presumed to be common Carry-over on this Period.

```
      DIMENSION W(8,49),WD(28,49),WV(49)
      DIMENSION SC(8),SR(40),RS(4)
      DIMENSION KPP(40,8),Q(49,49),V(49),RV(3,49)
      DIMENSION A(40),B(20),D(20),G(20)
      DIMENSION HED(3,3)
      DIMENSION DED(6,4),JSW(3)
      DIMENSION RESHED(9,2),RESSUB(3,3),LD(3,2),DF(3),LR(3,2),LTW(3),
     1LBW(3)
      REAL*4 KRPC(40,8),MU,WW(49,49)
      REAL*8 MID(3)
      DATA DED/    '                          ',
     1             ' DETERMINANT OVERFLOW   ',
     2             ' DETERMINANT UNDERFLOW  ',
     3             ' MATRIX WAS SINGULAR    '/
      DATA HED/'SANS DELTA  ','SANS GAMMA  ','FULL MATRIX '/
      DATA RESSUB/'            ','TREATMENTS  ','CARRIES-OVER'/
      DATA LD /1,2,2,2,3,3/,LR/4,2,1,1,3,3/,LTW/1,3,2/,LBW/2,2,3/
      DATA RESHED/ 'MU ROWS COLUMNS                  CONTROL',
     1            'CONTROL FACTORS PLUS                     '/
      DATA MID/'BETA','GAMMA','DELTA'/
C          KCTT = COLUMN + TREATMENT + CARRY-OVER
C          KCT1 = KCTT + 1
C          KPP (ROWS,COLUMNS)     PATTERN MATRIX
C          KRPC (ROWS,COLUMNS)     RESULTS MATRIX
C          W (COLUMNS,KCT1)     WINGERSKY MATRIX
C          WD ((KC*(KC-1)/2),KCT1)     DIFFERENCE MATRIX
C          WV (KCT1)     WINGERSKY VECTOR
C          Q (KCT1,KCT1)     WORK MATRIX
C          V (KCT1)     WORK VECTOR
C          RV (3,KCT1)     ESTIMATES VECTORS
C          A (ROW)     ROW COUNTS
C          B (COLUMNS)     COLUMN COUNTS
C          D (TREATMENTS)     TREATMENT COUNTS
C          G (CARRY OVER)     CARRY OVER COUNTS
C          WW (KCT1,KCT1)     WORK MATRIX
C          D (TEATMENTS)     TREATMENT COUNTS
      NPAGE=1
  101 WRITE(6,901) NPAGE
  901 FORMAT('1',///,' ANALYSIS OF CARRY OVER',T127,I3,
     1/,' GEOFFERY BEALL')
      NPAGE=NPAGE+1
      FORMAT(I1)
      KIN=5
      WRITE(6,12)
      FORMAT(1X,'PARAMETER CARD')
    2 FORMAT(10I5)
    3 FORMAT(10F5.1)
   16 FORMAT('+',60X,10F7.2)
      READ(KIN,1)KT,KC,KRC,KP
      FORMAT(5I5)
      WRITE(6,17) KT,KC,KRC,KP
      FORMAT(1X,5I5)
      DO 116 I=1,49
      DO 116 J=1,49
      DO 116 K=1,3
      RV(I,K)=0.
  116 WW(I,J)=0
      DO 117 I=1,8
  117 SC(I)=0
```

```
      DO 115 I=1,40                                                     CAR00440
      A(I)=0                                                            CAR00450
  115 SR(I)=0                                                           CAR00460
      KCTT=KC+2*KT                                                      CAR00470
      KCT1=KCTT+1                                                       CAR00480
      DP=0                                                              CAR00490
      DO 105 I=1,KP                                                     CAR00500
      READ(KIN,2)(KPP(I,J),J=1,KC)                                      CAR00510
      IF(KC.GT.5) GO TO 105                                             CAR00520
      IF(I.EQ.1) GO TO 105                                              CAR00530
      IK=I-1                                                            CAR00540
      DO 104 K=1,IK                                                     CAR00550
      DO 103 J=1,KC                                                     CAR00560
      IF(KPP(I,J).NE.KPP(K,J)) GO TO 104                                CAR00570
  103 CONTINUE                                                          CAR00580
      DP=DP+1                                                           CAR00590
      GO TO 105                                                         CAR00600
  104 CONTINUE                                                          CAR00610
  105 CONTINUE                                                          CAR00620
      IPC=KP-DP                                                         CAR00630
      WRITE(6,13)                                                       CAR00640
   13 FORMAT(' PATTERNS',52X,'RESULTS')                                 CAR00650
      DO 111 I=1,KP                                                     CAR00660
      READ(KIN,3)(KRPC(I,J),J=1,KC)                                     CAR00670
      DO 110 J=1,KC                                                     CAR00680
      SR(I)=SR(I)+KRPC(I,J)                                             CAR00690
  110 SC(J)=SC(J)+KRPC(I,J)                                             CAR00700
      WRITE(6,2)(KPP(I,J),J=1,KC)                                       CAR00710
  111 WRITE(6,16)(KRPC(I,J),J=1,KC)                                     CAR00720
      DO 198 I=1,49                                                     CAR00730
  198 WV(I)=0                                                           CAR00740
      DO 199 I=1,20                                                     CAR00750
      B(I)=0                                                            CAR00760
      D(I)=0                                                            CAR00770
  199 G(I)=0                                                            CAR00780
      C=0                                                               CAR00790
      TSA=0                                                             CAR00800
      TSSA=0                                                            CAR00810
      DO 135 K=1,KP                                                     CAR00820
      DO 122 J=1,49                                                     CAR00830
      DO 121 I=1,8                                                      CAR00840
  121 W(I,J)=0                                                          CAR00850
      DO 122 I=1,28                                                     CAR00860
  122 WD(I,J)=0                                                         CAR00870
      DO 125 I=1,KC                                                     CAR00880
      W(I,I)=1.                                                         CAR00890
      W(I,KPP(K,I)+KC)=1.                                               CAR00900
      IF(I.EQ.1) GO TO 125                                              CAR00910
      W(I,KPP(K,I-1)+KC+KT)=1.                                          CAR00920
  125 W(I,KCT1)=KRPC(K,I)                                               CAR00930
      KCM1=KC-1                                                         CAR00940
      N1=1                                                              CAR00950
      LA=0                                                              CAR00960
      DO 130 I=1,KCM1                                                   CAR00970
      N1=N1+1                                                           CAR00980
      DO 130 J=N1,KC                                                    CAR00990
      LA=LA+1                                                           CAR01000
      DO 130 L=1,KCT1                                                   CAR01010
  130 WD(LA,L)=W(I,L)-W(J,L)                                            CAR01020
      DO 134 I=1,LA                                                     CAR01030
      DO 134 J=1,KCTT
      DO 134 L=1,KCT1
  134 WW(J,L)=WW(J,L)+WD(I,J)*WD(I,L)
  135 CONTINUE
C     CONDITIONING DIFFERECCE MATRIX
      DO 140 I=1,KCT1
      WW(KC,I)=0
      WW(KC+KT,I)=0
  140 WW(KCTT,I)=0
      N1=1
      DO 150 I=KC,KCTT,KT
      DO 149 J=N1,I
  149 WW(I,J)=1.
  150 N1=I+1
      N=KC+KT+1
      DO 175 I=1,49
      DO 175 J=1,49
  175 Q(I,J)=0
      DO 190 K=1,3
      IF((K.EQ.3).AND.((IPC*KC).LT.(KC+2*KT-1))) GO TO 260
      DO 185 I=1,KCTT
      DO 185 J=1,KCT1
  185 Q(I,J)=WW(I,J)
      IF(K.EQ.3) GO TO 181
      M=N+KT-1
      DO 180 I=N,M
      DO 180 J=1,KCT1
      Q(I,J)=0
  .80 Q(I,I)=1.
      N=N-KT
  .81 DO 192 I=1,KCTT
  .92 V(I)=Q(I,KCT1)
      CALL LINEQ(KCTT,Q,V,1,DET,MORT,49)
  .89 JSW(K)=MORT+1
      IF(MORT.NE.0) GO TO 186
      DO 188 I=1,KCTT
  .88 RV(K,I)=V(I)
      GO TO 190
  261 FORMAT(//,' INADEQUATE FOR CARRY OVER')
  260 WRITE(6,261)
      JSW(3)=4
   86 DO 187 I=1,KCTT
   87 RV(K,I)=0
      CONTINUE
      WRITE(6,902) NPAGE
   02 FORMAT('1',///,' ESTIMATES OF VARIABLES',T127,I3)
      NPAGE=NPAGE+1
      N1=1
      N2=KC
      WRITE(6,203)((HED(I,J),I=1,3),J=1,3,2)
   03 FORMAT(///,2(28X,3A4),//)
      DO 209 I=1,3
      WRITE(6,205) MID(I)
   05 FORMAT(52X,A8)
      DO 206 J=N1,N2
   06 WRITE(6,204)RV(1,J),RV(3,J)
   04 FORMAT(2(28X,F12.4))
      N1=N2+1
   09 N2=N2+KT
```

```
      WRITE(6,207)((DED(L,JSW(K)),L=1,6),K=1,3,2)
  207 FORMAT(18X,6A4,14X,6A4)
      DO 200 I=1,KP
      DO 200 J=1,KC
      IF(KPP(I,J).EQ.0) GO TO 200
      WV(J)=WV(J)+KRPC(I,J)
      WV(KPP(I,J)+KC)=WV(KPP(I,J)+KC)+KRPC(I,J)
      IF(J.EQ.1) GO TO 201
      WV(KPP(I,J-1)+KC+KT)=WV(KPP(I,J-1)+KC+KT)+KRPC(I,J)
  201 TSA=TSA+KRPC(I,J)
      TSSA=TSSA+(KRPC(I,J)*KRPC(I,J))
      C=C+1.
      A(I)=A(I)+1.
      B(J)=B(J)+1.
      G(KPP(I,J))=G(KPP(I,J))+1.
      IF(J.EQ.1) GO TO 200
      D(KPP(I,J-1))=D(KPP(I,J-1))+1.
  200 CONTINUE
      DO 300 K=1,3
      IF(JSW(K).NE.1) GO TO 300
      TSS=TSSA
      TS=TSA
      DO 210 I=1,KCTT
  210 TSS=TSS-(RV(K,I)*WV(I))
      MU=0
      DO 220 I=1,KT
  220 MU=G(I)*RV(K,I+KC)+D(I)*RV(K,KC+KT+I)+MU
      MU=(TS-MU)/C
      TSS=TSS-MU*TS
      DO 250 I=1,KP
      SA=0
      SB=0
      DO 230 J=1,KC
      SB=SB+KRPC(I,J)
      IF(J.EQ.1) GO TO 225
      SA=KRPC(I,J)-RV(K,KC+KPP(I,J))-RV(K,KC+KT+KPP(I,J-1))-MU+SA
      GO TO 230
  225 SA=KRPC(I,J)-RV(K,(KC+KPP(I,J)))-MU+SA
  230  CONTINUE
      SA=(SA/A(I))*SB
  250 TSS=TSS-SA
      RS(K)=TSS
  300 CONTINUE
      FKP=FLOAT(KP)
      FKC=FLOAT(KC)
      FKT=FLOAT(KT)
      SSC=0
      DO 310 I=1,KC
  310 SSC=SSC+SC(I)**2/FKP
      SSR=0
      DO 320 I=1,KP
  320 SSR=SSR+SR(I)**2/FKC
      RS(4)=TSSA+((TSA*TSA)/C)-SSC-SSR
      DF(1)=C-FKP-FKC+1.
      DF(2)=C-FKP-FKC-FKT+2.
      DF(3)=C-FKP-FKC-2.*FKT+3.
      WRITE(6,903) NPAGE
  903 FORMAT('1',///,' ANALYSIS OF VARIANCE',T127,I3)
      NPAGE=NPAGE+1
      DO 400 K=1,3
```

```
    IF(JSW(K).NE.1) GO TO 390                                           CAR02230
    WRITE(6,331)                                                        CAR02240
331 FORMAT(///,19X,'FACTORS',18X,'| D.F. |','  RESIDUAL VARIABILITY')   CAR02250
    WRITE(6,332)                                                        CAR02260
332 FORMAT(' ---------------------------------------------+------+------CAR02270
   1------------------')                                                CAR02280
    DO 335 J=1,3                                                        CAR02290
    RESHED(4+J,1)=RESSUB(J,LTW(K))                                      CAR02300
335 RESHED(6+J,2)=RESSUB(J,LBW(K))                                      CAR02310
    DO 338 J=1,2                                                        CAR02320
338 WRITE(6,333)(RESHED(I,J),I=1,9),DF(LD(K,J)),RS(LR(K,J))             CAR02330
333 FORMAT(4X,9A4,4X,'|',F4.0,'   |',2X,F10.2)                          CAR02340
    WRITE(6,332)                                                        CAR02350
    DFF=DF(LD(K,1))-DF(LD(K,2))                                         CAR02360
    RE=((RS(LR(K,1))-RS(LR(K,2)))*DF(LD(K,2)))/(RS(LR(K,2))*DFF)        CAR02370
    WRITE(6,336)  RS(LR(K,1)),RS(LR(K,2)),DFF,RE                        CAR02380
336 FORMAT(//,18X,'(',F10.2,' - ',F8.2,' )/',F3.0,6X,F5.2)              CAR02390
    WRITE(6,334)                                                        CAR02400
334 FORMAT('+', 7X,'F',7X,'=_______________________________ =')         CAR02410
    WRITE(6,337) DFF,DF(LD(K,2)),RS(LR(K,2)),DF(LD(K,2))                CAR02420
337 FORMAT( 9X,F3.0,',',F3.0, 8X,F8.2,' / ',F3.0)                       CAR02430
    IF(JSW(3).NE.1) GO TO 501                                           CAR02440
    GO TO 400                                                           CAR02450
390 WRITE(6,391)(DED(L,JSW(K)),L=1,6)                                   CAR02460
391 FORMAT(//,1X,6A4,//)                                                CAR02470
400 CONTINUE                                                            CAR02480
501 READ(5,26) IT                                                       CAR02490
    IF(IT.EQ.1) GO TO 101                                               CAR02500
    FORMAT(1X,17F4.1)                                                   CAR02510
    FORMAT(1X,9F8.3)                                                    CAR02520
    FORMAT(1X,F12.4)                                                    CAR02530
    CALL EXIT                                                           CAR02540
    END                                                                 CAR02550
```

```
      SUBROUTINE LINEQ(N,A,B,M,DET,MORT,NR)
      DIMENSION A(NR,NR),B(NR,1), PIVOT(53),IPIV(53),INDEX(53,2)
      EQUIVALENCE (IROW,JROW),(ICOL,JCOL),(AMAX,T,SWAP)
      MORT=0
      IND=2
      DET=1.0
      DO 20 J=1,N
20    IPIV(J)=0
      DO 550 I=1,N
      AMAX=0.0
      DO 105 J=1,N
      IF(IPIV(J)-1)60,105,60
60    DO 100 K=1,N
      IF(IPIV(K)-1)80,100,720
80    IF(ABS(AMAX)-ABS(A(J,K)))85,100,100
85    IROW=J
      ICOL=K
      AMAX=A(J,K)
100   CONTINUE
105   CONTINUE
      IPIV(ICOL)=IPIV(ICOL)+1
      IF(IPIV(ICOL)-1)720,130,720
130   IF(IROW-ICOL)140,260,140
140   DET=-DET
      DO 200 L=1,N
      SWAP=A(IROW,L)
      A(IROW,L)=A(ICOL,L)
200   A(ICOL,L)=SWAP
      IF(M)260,260,210
210   DO 250 L=1,M
      SWAP=B(IROW,L)
      B(IROW,L)=B(ICOL,L)
250   B(ICOL,L)=SWAP
260   INDEX(I,1)=IROW
      INDEX(I,2)=ICOL
      PIVOT(I)=A(ICOL,ICOL)
      IF(ABS(PIVOT(I)).LT..0001)GO TO 720
      DET=DET*PIVOT(I)
      IF(ABS(DET)-1.E36)325,712,712
325   IF(ABS(DET)-1.E-36)715,715,330
330   A(ICOL,ICOL)=1.0
      DO 350 L=1,N
350   A(ICOL,L)=A(ICOL,L)/PIVOT(I)
      IF(M)380,380,360
360   DO 370 L=1,M
370   B(ICOL,L)=B(ICOL,L)/PIVOT(I)
380   DO 550 L1=1,N
      IF(L1-ICOL)400,550,400
400   T=A(L1,ICOL)
      A(L1,ICOL)=0.0
      DO 450 L=1,N
450   A(L1,L)=A(L1,L)-A(ICOL,L)*T
      IF(M)550,550,460
460   DO 500 L=1,M
500   B(L1,L)=B(L1,L)-B(ICOL,L)*T
550   CONTINUE
      DO 710 I=1,N
      L=N+1-I
      IF(INDEX(L,1)-INDEX(L,2))630,710,630
```

```
30    JROW=INDEX(L,1)
      JCOL=INDEX(L,2)
      DO 705 K=1,N
      SWAP=A(K,JROW)
      A(K,JROW)=A(K,JCOL)
      A(K,JCOL)=SWAP
05    CONTINUE
10    CONTINUE
      RETURN
  712 MORT=1
      RETURN
  715 MORT=2
      RETURN
  720 MORT=3
      RETURN
      END
```

There follows the program for an experiment with a conditioning Period, i.e., one where the first Period of experimental results is preceded by one to control the Change-over. This program uses the same SUBROUTINE LINEQ as the preceding, but it is not shown again.

```
      DIMENSION W(8,53),WD(28,53) ,WV(53)
      DIMENSION                     SC(8),SR(40),RS( 4)
      DIMENSION KPP(40,8),Q(53,53),V(53),RV(3,53),KPC(40,9)
      DIMENSION A(40),B(20),D(20),G(20)
      DIMENSION HED(3,3)
      DIMENSION DED(6,4),JSW(3)
      DIMENSION RESHED(9,2),RESSUB(3,3),LD(3,2),DF(3),LR(3,2),LTW(3),
     1LBW(3)
      REAL*4 KRPC(40,8),MU,WW(53,53)
      REAL*8 MID(3)
      EQUIVALENCE (KPC(1,2),KPP(1,1))
      DATA MID/'BETA    ','GAMMA   ','DELTA   '/
      DATA DED/    '                        ',
     1             ' DETERMINANT OVERFLOW   ',
     2             ' DETERMINANT UNDERFLOW  ',
     3             ' MATRIX HAS SINGULAR    '/
      DATA HED/'SANS DELTA  ','SANS GAMMA  ','FULL MATRIX '/
      DATA RESSUB/'            ','TREATMENTS  ','CARRIES-OVER'/
      DATA LD /1,2,2,2,3,3/,LR/4,2,1,1,3,3/,LTW/1,3,2/,LBW/2,2,3/
      DATA RESHED/ 'MU ROWS COLUMNS                      CONTROL',
     1             'CONTROL FACTORS PLUS                        '/
C
C               KT = NO. OF TREATMENTS
C               KC = NO. OF COLUMS OF RESULTS
C               KCP = NO. OF COLUMNS IN PATTERN
C               KP=  NO. OF ROWS
C
C
C        KCTT = KC + KT + KT
C        KCT1 = KCTT + 1
C        KPP (KP,KC)     PATTERNS MATRIX
C        KPC (KCP,KC)     PATTERNS MATRIX WITH CONDITIONING WEEK
C        KRPC (KP,KC)     RESULTS MATRIX
C        W (KC,KCT1)    WINGERSKY MATRIX
C        WD ((KC*(KC-1)/2),KCT1)    DIFFERENCE MATRIX
C        WV (KCT1)     WINGERSKY MATRIX
C        Q (KCT1,KCT1)     WORK MATRIX
C        V (KCT1)    WORK VECTOR
C        SC (KC)    SUM OF COLUMNS
C        SR(KP)    SUM OF ROWS
C        A (KP)    ROW COUNTS
C        B (KC)    COLUMN COUNTS
C        D (KT)    TREATMENT COUNTS
C        G (KT)    CARRY OVER COUNTS
C        RV (3,KCT1)    STORES ESTIMATES
C        WW (KCT1,KCT1)     WORK MATRIX
      NPAGE=1
  101 WRITE(6,901) NPAGE
  901 FORMAT('1',///,' ANALYSIS OF CARRY-OVER WITH CONDITION WEEK',T127,
     1I3,/,' GEOFFERY BEALL',//)
      NPAGE=NPAGE+1
      FORMAT(I1)
      KIN=5
      WRITE(6,12)
      FORMAT(1X,'PARAMETER CARD')
    2 FORMAT(10I5)
    3 FORMAT(10F5.1)
   16 FORMAT('+',60X,10F6.2)
      READ(KIN,1)KT,KC,KCP,KP
      FORMAT(5I5)
      WRITE(6,17) KT,KC,KCP,KP
```

```
7     FORMAT(1X,5I5)
      DO 116 I=1,53
      DO 116 J=1,53
      DO 116 K=1,3
      RV(I,K)=0.
  116 WW(I,J)=0
      DO 117 I=1,8
  117 SC(I)=0
      DO 115 I=1,40
      A(I)=0
  115 SR(I)=0
      KCTT=KC+2*KT
      KCT1=KCTT+1
      DP=0
      DO 105 I=1,KP
      READ(KIN,2)(KPC(I,J),J=1,KCP)
      IF(KCP.GT.5) GO TO 105
      IF(I.EQ.1) GO TO 105
      IK=I-1
      DO 104 K=1,IK
      DO 103 J=1,KCP
      IF(KPC(I,J).NE.KPC(K,J)) GO TO 104
  103 CONTINUE
      DP=DP+1
      GO TO 105
  104 CONTINUE
  105 CONTINUE
      IPC=KP-DP
      WRITE(6,13)
   13 FORMAT(' PATTERNS',52X,'RESULTS')
      DO 111 I=1,KP
      READ(KIN,3)(KRPC(I,J),J=1,KC)
      DO 110 J=1,KC
      SR(I)=SR(I)+KRPC(I,J)
  110 SC(J)=SC(J)+KRPC(I,J)
      WRITE(6,2)(KPC(I,J),J=1,KCP)
  111 WRITE(6,16)(KRPC(I,J),J=1,KC)
      WRITE(6,14) IPC
   14 FORMAT(//,' NUMBER OF UNIQUE PATTERNS',I8)
      DO 198 I=1,53
  198 WV(I)=0
      DO 199 I=1,20
      B(I)=0
      D(I)=0
  199 G(I)=0
      C=0
      TSA=0
      TSSA=0
      DO 135 K=1,KP
      DO 122 J=1,53
      DO 121 I=1,8
  121 W(I,J)=0
      DO 122 I=1,28
  122 WD(I,J)=0
      DO 125 I=1,KC
      W(I,I)=1.
      W(I,KPP(K,I)+KC)=1.
      W(I,KPC(K,I)+KC+KT)=1.
  125 W(I,KCT1)=KRPC(K,I)
```

```
      KCM1=KC-1
      N1=1
      LA=0
      DO 130 I=1,KCM1
      N1=N1+1
      DO 130 J=N1,KC
      LA=LA+1
      DO 130 L=1,KCT1
130   WD(LA,L)=W(I,L)-W(J,L)
      DO 134 I=1,LA
      DO 134 J=1,KCTT
      DO 134 L=1,KCT1
134   WW(J,L)=WW(J,L)+WD(I,J)*WD(I,L)
135   CONTINUE
C     CONDITIONING DIFFERECCE MATRIX
      DO 140 I=1,KCT1
      WW(KC,I)=0
      WW(KC+KT,I)=0
 40   WW(KCTT,I)=0
      N1=1
      DO 150 I=KC,KCTT,KT
      DO 149 J=N1,I
 49   WW(I,J)=1.
 50   N1=I+1
      N=KC+KT+1
      DO 175 I=1,53
      DO 175 J=1,53
 75   Q(I,J)=0
      DO 190 K=1,3
      IF((K.EQ.3).AND.((IPC*KC).LT.(KCTT-1))) GO TO 260
      DO 185 I=1,KCTT
      DO 185 J=1,KCT1
 85   Q(I,J)=WW(I,J)
      IF(K.EQ.3) GO TO 181
      M=N+KT-1
      DO 180 I=N,M
      DO 180 J=1,KCT1
      Q(I,J)=0
 80   Q(I,I)=1.
      N=N-KT
 81   DO 192 I=1,KCTT
 92   V(I)=Q(I,KCT1)
      CALL LINEQ(KCTT,Q,V,1,DET,MORT,53)
 89   JSW(K)=MORT+1
      IF(MORT.NE.0) GO TO 186
      DO 188 I=1,KCTT
 88   RV(K,I)=V(I)
      GO TO 190
 60   WRITE(6,261)
 61   FORMAT(//,' INADEQUATE FOR CARRY-OVER')
      JSW(3)=4
 86   DO 187 I=1,KCTT
 87   RV(K,I)=0
      CONTINUE
      WRITE(6,902) NPAGE
 02   FORMAT('1',///,' ESTIMATES OF VARIABLES',T127,I3)
      NPAGE=NPAGE+1
 91   WRITE(6,203)((HED(I,K),I=1,3),K=1,3,2)
 03   FORMAT(///,2(28X,3A4),//)
      N1=1
      N2=KC
      DO 209 I=1,3
      WRITE(6,205) MID(I)
  205 FORMAT(52X,A8)
      DO 206 N=N1,N2
  206 WRITE(6,204) RV(1,N),RV(3,N)
      N1=N2+1
  209 N2=N2+KT
  204 FORMAT(2(28X,F12.4))
      WRITE(6,401)((DED(L,JSW(I)),L=1,6),I=1,3,2)
  401 FORMAT(/,2(16X,6A4))
      DO 200 I=1,KP
      DO 200 J=1,KC
      IF(KPP(I,J).EQ.0) GO TO 200
      WV(J)=WV(J)+KRPC(I,J)
      WV(KPP(I,J)+KC)=WV(KPP(I,J)+KC)+KRPC(I,J)
      WV(KPC(I,J  )+KC+KT)=WV(KPC(I,J  )+KC+KT)+KRPC(I,J)
  201 TSA=TSA+KRPC(I,J)
      TSSA=TSSA+(KRPC(I,J)*KRPC(I,J))
      C=C+1.
      A(I)=A(I)+1.
      B(J)=B(J)+1.
      G(KPP(I,J))=G(KPP(I,J))+1.
      D(KPC(I,J  ))=D(KPC(I,J  ))+1.
  200 CONTINUE
      DO 300 K=1,3
      IF(JSW(K).NE.1) GO TO 300
      TSS=TSSA
      TS=TSA
      DO 210 I=1,KCTT
  210 TSS=TSS-(RV(K,I)*WV(I))
      MU=0
      DO 220 I=1,KT
  220 MU=G(I)*RV(K,I+KC)+D(I)*RV(K,KC+KT+I)+MU
      MU=(TS-MU)/C
      TSS=TSS-MU*TS
      DO 250 I=1,KP
      SA=0
      SB=0
      DO 230 J=1,KC
      SB=SB+KRPC(I,J)
      SA=KRPC(I,J)-RV(K,KC+KPP(I,J))-RV(K,KC+KT+KPC(I,J  ))-MU+SA
  230  CONTINUE
      SA=(SA/A(I))*SB
   50 TSS=TSS-SA
      RS(K)=TSS
  300 CONTINUE
      FKP=FLOAT(KP)
      FKC=FLOAT(KC)
      FKT=FLOAT(KT)
      SSC=0
      DO 310 I=1,KC
  310 SSC=SSC+SC(I)**2/FKP
      SSR=0
      DO 320 I=1,KP
  320 SSR=SSR+SR(I)**2/FKC
      RS(4)=TSSA+((TSA*TSA)/C)-SSC-SSR
      DF(1)=C-FKP-FKC+1.
      DF(2)=C-FKP-FKC-FKT+2.
```

```
    DF(3)=C-FKP-FKC-2.*FKT+3.
    WRITE(6,903) NPAGE
903 FORMAT('1',///' ANALYSIS OF VARIANCE' ,T127,I3)
    NPAFE=NPAGE+1
    DO 400 K=1,3
    IF(JSW(K).NE.1) GO TO 390
    WRITE(6,331)
331 FORMAT(///,19X,'FACTORS',18X,'| D.F. |','  RESIDUAL VARIABILITY')
    WRITE(6,332)
332 FORMAT(' -------------------------------------------+------+------
   1------------------')
    DO 335 J=1,3
    RESHED(4+J,1)=RESSUB(J,LTW(K))
335 RESHED(6+J,2)=RESSUB(J,LBW(K))
    DO 338 J=1,2
338 WRITE(6,333)(RESHED(I,J),I=1,9),DF(LD(K,J)),RS(LR(K,J))
333 FORMAT(4X,9A4,4X,'|',F4.0,'  |',2X,F10.2)
    WRITE(6,332)
    DFF=DF(LD(K,1))-DF(LD(K,2))
    RE=((RS(LR(K,1))-RS(LR(K,2)))/(DFF))/(RS(LR(K,2))/DF(LD(K,2)))
    WRITE(6,336)  RS(LR(K,1)),RS(LR(K,2)),DFF,RE
336 FORMAT(//,21X,'(',F8.2,' - ',F8.2,' )/',F3.0,6X,F5.2)
    WRITE(6,334)
334 FORMAT('+', 7X,'F',7X,'=______________________________ =')
    WRITE(6,337) DFF,DF(LD(K,2)),RS(LR(K,2)),DF(LD(K,2))
337 FORMAT( 9X,F3.0,',',F3.0, 8X,F8.2,' / ',F3.0)
    IF(JSW(3).NE.1) GO TO 501
    GO TO 400
390 WRITE(6,391)(DED(L,JSW(K)),L=1,6)
391 FORMAT(//,1X,6A4,//)
400 CONTINUE
501 READ(5,26) IT
    IF(IT.EQ.1) GO TO 101
    FORMAT(1X,17F4.1)
    FORMAT(1X,9F8.3)
    FORMAT(1X,F12.4)
    CALL EXIT
    END
```